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1. Advanced Psychology. Professor Münsterberg. First half year.

2. Experimental Psychology. Mr. Lough.

3. Philosophy of Nature. Lotze's Microcosmus. Professor James.

4. Ethics. Professor Palmer.

5. The Ethics of the Social Question. Professor Peabody.

6. The Psychological Elements of Religious Faith. Professor Everett. Once a week.

7. Theism and the Special Contents of Christian Faith. Professor Everett.

8. Metaphysics. Deussen's outlines. Bradley's Appearance and Reality. Professor Royce.

9. The Nervous System and its Terminal Organs. Dr. Parker.

Group III.—Historical Courses.

1. Greek Philosophy—in translation. Dr. Santayana.

2. PLATO'S REPUBLIC. ARISTOTLE'S ETHICS. Professor GOODWIN.

3. ENGLISH PHILOSOPHY FROM LOCKE TO HUME. Dr. SANTAYANA. *First half year.*

4. DESCARTES, SPINOZA AND LEIBNITZ. Dr. SANTAYANA. *Second half year.*

5. KANT'S PHILOSOPHY. Professor JAMES. *First half year.*

6. COMPARATIVE RELIGION. Professor EVERETT. *Twice weekly.*

7. THE ALGEBRA OF LOGIC. Professor J. M. PEIRCE. *One hour a week.*

Group IV.—Courses of Research.

1. Psychological Laboratory Investigations and Lectures on Methods of Research. Professor MÜNSTERBERG.

2. PSYCHOLOGICAL SEMINARY. Speculative Problems. Professor JAMES.

3. METAPHYSICAL SEMINARY. Hegel's System. Professor ROYCE.

4. ETHICAL SEMINARY. Historical Development of Ethics in England. Professor PALMER.

5. SOCIOLOGICAL SEMINARY. The Christian Doctrine of the Social Order. Professor PEABODY.

6. STUDIES IN SCHOLASTIC PHILOSOPHY. Dr. SANTAYANA.

Group V.—Courses in Education and Teaching.

1. THE HISTORY OF EDUCATIONAL THEORIES AND PRACTICES.

2. INTRODUCTION TO EDUCATIONAL THEORY.

3. METHODS OF TEACHING MATHEMATICS, PHYSICS, CHEMISTRY, PHYSIOGRAPHY, METEOROLOGY AND PHYSIOLOGY IN SCHOOLS.

4. THE ORGANIZATION AND MANAGEMENT OF PUBLIC SCHOOLS AND ACADEMIES.

Professor HANUS being absent next year, these four course will be under the direction of a substitute not yet appointed (April, 1897).

Courses in Philosophy Open to Graduate Students in Princeton University (1897-8).

I. Moral and Religious Philosophy.

1. ETHICS. Theoretical and practical ethics, the foundation of moral obligation, the will, conscience, the nature of virtue, and the moral law. Recent ethical discussions. Lectures. Senior Required; first term [2].* The President. *Calderwood:* Handbook of Moral Philosophy.

5, 6. (LATIN 13, 14.) Lucretius, De Rerum Natura, and Cicero, De Natura Deorum. Senior Elective; both terms [2]. Professor PACKARD.

7, 8. THEISM. Senior Elective; both terms [2]. The President.

9. SCIENCE AND RELIGION. The history and the logic of the sciences with reference to emerging problems of religion. Graduate course; second term [2] Professor SHIELDS. Lectures.

II. Mental Philosophy.

3 I. HISTORY OF ANCIENT PHILOSOPHY. Greek and Roman Philosophy to close of Pagan Schools. Given 1897–98, alternating with 3 II. Junior and Senior Elective; first term [2]. Professor ORMOND. Lectures. *Zeller:* Outlines of Greek Philosophy; *Windelband:* History of Philosophy.

4 I. HISTORY OF MEDIEVAL PHILOSOPHY, embracing the patristic and scholastic periods and ending with Francis Bacon. Given 1897–98, alternating with 4 II. Junior and Senior Elective; second term [2]. Professor ORMOND. Lectures, with references to *Ueberweg, Erdmann, Windelband.*

3 II. HISTORY OF MODERN PHILOSOPHY. Philosophy from Descartes to Kant. Given 1896–97, alternating with 3 I. Junior and Senior Elective; first term [2]. Professor ORMOND. Lectures, with references to *Falckenberg.*

4 II. HISTORY OF MODERN PHILOSOPHY. Philosophy since Kant. Given 1896–97, alternating with 4 I. Junior and Senior Elective; second term [2]. Professor ORMOND. Lectures, with references to *Falckenberg.*

The courses 3 I, 4 I, 3 II, 4 II furnish a continuous course of lectures on historical philosophy running through two years.

6. EXPERIMENTAL PSYCHOLOGY. Introduction to the subject. The method, scope, and most general results of the experimental treatment of the mind, considered especially in its connection with general psychology, education, and medicine. Special demonstrations, in the laboratory, of the psychology of movement and sensation. Junior Elective; second term [2], Professor WARREN and Mr. CRAWFORD. *Ribot:* German Psychology of To-day. *Sanford:* Course in Experimental Psychology.

8. (GREEK 10.) The Protagoras of Plato, and lectures on the Platonic philosophy. Junior and Senior Elective; second term [2]. Professor ORRIS.

* Numbers in brackets indicate hours per week.

9. INDUCTION AND THEORY OF LOGIC. Senior Elective; first term [2]. Professor HIBBEN. Lectures.

10. SYMBOLIC LOGIC AND THEORY OF PROBABILITY. Senior Elective second term [2]. Professor HIBBEN. Lectures.

11. (GREEK 13.) Aristotle, the Nicomachean Ethics, with prolegomena and dictations. Senior Elective; first term [2]. Professor ORRIS.

13. METAPHYSICS AND THEORY OF KNOWLEDGE. Lectures. Senior Elective; first term [2]. Professor ORMOND. *McCosh:* First and Fundamental Truths.

14. OUTLINES OF PHILOSOPHY. A course in encyclopedia of philosophy. Senior Elective; second term [2]. Professor ORMOND. Lectures.

15. PHYSIOLOGICAL PSYCHOLOGY. Lectures and laboratory work on the anatomy and physiology of the nervous system in their bearing upon the problems of psychology. Senior Elective; first term [2]. Professors SCOTT and MCCLURE, and Mr. DAHLGREN. *Ladd:* Physiological Psychology.

16. THE PSYCHOLOGY OF LOGIC. Senior elective; second term [2]. Professor WARREN.

17. EXPERIMENTAL PSYCHOLOGY. A continuation of course 6, although independent in its topics. The psychology of the special senses (sight, hearing, touch, etc.) experimentally treated. Senior Elective; first term [2]. Professors BALDWIN and WARREN. Lectures, demonstrations, and practical work required of all students in this course.

18. EXPERIMENTAL PSYCHOLOGY. Detailed treatment by lectures and demonstrations of the measurement of mental intensities (Weber's law) and of the results of mental chronometry. Senior Elective; first term [2], Professors BALDWIN and WARREN and Mr. CRAWFORD. References: Wundt, Külpe, Jastrow, Cattell.

19, 20. GENERAL PSYCHOLOGY. Advanced course. First term: Senses and Intellect; second term: Feeling and Will; with consideration of abnormal mental conditions. Senior Elective, open to graduates; both terms [2]. Professor BALDWIN. *James, Hoffding, Ladd, Baldwin.*

21, 22. SELECTED TOPICS IN PHILOSOPHY. Graduate course; both terms [2]. Professor ORMOND. Lectures.

23, 24. (GREEK 15, 16.) Plato; analyses of his dialogues, lectures on his philosophy, reading of the Phædo and parts of the Republic. Graduate course; both terms [1]. Professor ORRIS.

25, 26. EXPERIMENTAL PSYCHOLOGY. Consisting largely in research work. Graduate course; both terms [2]. Professors BALDWIN and WARREN and Mr. CRAWFORD.

27, 28. MODERN AND CONTEMPORARY PHILOSOPHY. Graduate Seminary; both terms [2]. Readings, discussions and theses. This course is also open as an elective to those Seniors who are candidates for Special Honors in Philosophy. Professor ORMOND.

29, 30. GRADUATE PSYCHOLOGICAL SEMINARY. Open as an optional to Seniors of the first and second groups who elect 19 or 20; both terms [1]. Professor BALDWIN. Subject for 1897–98: Social and Religious Psychology.

31. THEORY OF MENTAL MEASUREMENTS. Graduate course; first term. Professor WARREN.

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THE PSYCHOLOGICAL REVIEW.

THE NEGATIVE IN LOGIC.[1]

BY PROFESSOR A. T. ORMOND.

Princeton University.

Historically the negative has occupied the attention of logicians since the first beginnings of the science. Aristotle gave it a prominent place in his reflection and in modern times it has been discussed by all the masters in this field; by Leibnitz and Kant, by Hamilton, Lotze, Sigwart, Wundt, Bradley, Bosanquet and Benno Erdmann. It is not my purpose in this paper to review the work of these thinkers, even in outline, but rather with their results in mind to attempt a statement of what I conceive to be the most important features of an adequate theory of logical negation. In the first place it is clear, I think, that the logical negative is very closely implicated in the general theory of judgment and that a radical treatment of it must go to the roots of judgment itself. For this reason a considerable section of this paper will be devoted to judgment with a view to seeking its psychological and logical grounds.

In treating judgment psychologically we must conceive it as a conscious function, and this, followed back to its very first presupposition, would involve the question of the origin of the consciousness in which the function arises. But whatever the responsibility of psychology may be for the genesis of consciousness, logic is free I think to assume the medium in which the functions it is interested in are found. A question, however, which does, indirectly at least, concern the foundations of logic

[1] Read in abstract at the Boston meeting, American Psychological Association. The discussion is mainly psychological.

is that of the organic conditions in which consciousness operates. The tendency of the genetic thinking of the time is to go back of the psychological to the biological in order to discover the first laws or conditions of conscious activity. And this is, I think, on the whole, a healthy disposition, inasmuch as the vital and the psychic activities cannot be separated in an organism which has once become the bearer of consciousness. I do not mean that there are none of the activities of such an organism that are not psychic, but that within the circle of the conscious, the vital and the psychic are one and the same. To omit all detail, the important question here is, how, for our purposes, shall the relation of the biological to the psychic be conceived? There are two view-points that are to be kept separated in our thinking: the external or physical and the internal or psychic. From the physical standpoint, which is the biological, we view consciousness as something in the organism and superadded to the organic functions, whereas, from the psychic point of view, which is that of mind itself, consciousness is not simply in an organism or an appendage to its activities, but it is a comprehending term, the medium in which the existence of everything is realized, and in which the organism, in order to get itself recognized among existent things, must somehow become immanent as part of its content. Realizing this point of view we will be led to regard that duality which constitutes the mould of organic activity in general, the interaction of organism and environment, as immanent and structural in the sphere of conscious activity, and from the same point of view the biological laws of habit and accommodation will become immanent laws of consciousness. I mean by this that consciousness does not simply contemplate the biological functions as external and conditional to its own activities, but that when conscious activity arises, say, as will, the laws of habit and accommodation are taken up into consciousness and become constitutional principles of volitional activity.

Assuming, then, that the vital conditions the psychic in its own sphere by becoming immanent and constitutional to it, our notion of psychic activity will resolve itself into that of *conscious* vital function transforming and yet obeying the life categories

which have been taken up into the conscious sphere as immanent laws of psychic activity. Nor will it be difficult from this point of view to realize the ground on which the first conscious activity may be characterized as volitional, inasmuch as it will take the form of conscious reaction of the organism upon its environment, which, operating under the laws of habit and accommodation, it gradually assimilates and absorbs into itself. The general concept which I have sought to emphasize at this point is the immanence of the vital from the psychic point of view and the consequent necessity for translating the biological categories into internal and structural principles of the conscious activities.

If we regard the conscious organism simply as acted upon by its environment, that is, as a recipient of stimulations, there is no ground for ascribing will to it. It is only when we conceive it as active and as reacting upon the ground of stimulation that we can think of it as will. What we call will can, in these early psychic activities, be nothing but the conscious responses by which the organism effects its assimilative and adaptive movements. We may call them *pulses of self-assertion*, by which the organism wreaks itself upon the ground of stimulation, and the acts will be acts of self-conservation and will fall under the general category of survival.

These earlier acts of volition will not be primarily motived by any idea or representation, but rather by some feeling of pleasure or pain, most likely one of pain, since mere pleasure feeling could not serve as a motive for activity, but, on the contrary, in itself and without some accompanying idea or representation which would translate it into teleological terms, would tend to arrest motion. If we assume then that the very first motive-impulse, logically considered, is painful feeling we may conceive the primal impulse of volition as some want or unsatisfactory condition which *impels* the conscious organism to escape from its present state into one that shall be less intolerable. We have then the conception of a will motived in a negative sense from behind, but, so far as this element of motivity is concerned, blind as to what is before it and having no other guidance than the specific quality of the painful impulse, to enable it to pick its way among

the pleasant and painful stimulations of the environment. But by the pain-motive, which is a principle of *avoidance*, we may conceive the organism as feeling its way with a certain degree of selective intelligence, it being understood that the pleasure-motive becomes also active, and that representation when it arises attaches itself to both pleasure and pain as a teleological principle of positive and negative selection.

The will of such an organism would be an active function of appropriation and avoidance moving under the guidance of the selective motives, and the special question which arises here from our point of view is how the volitional activity comes to take on an intellectual character and become what we call judgment. To answer such a question in detail would involve a wide excursion into genetic psychology. The following statement must suffice at this point. The conscious organism not only collides volitionally with the grounds of its stimulations, but out of these collisions arise representations (the spatial no doubt arising first) which are to be conceived as elements of form under what these grounds appear to us as *objective* and *intelligible*. Let us suppose this process as completing itself in the presentation to consciousness of the objects of a world in the midst of which its functions are performed. So that what was blindly and vaguely realized before through feeling now stands out in a representation. If from this representation we subtract the volitional pulse we have simply a world presented but not affirmed. But if we restore this pulse as a conscious reaction upon the presentation we have the simplest assertion of the object; that is, judgment in its most elemental form. This is essential. In all judgment the central thing is a volitional pulse. To this as genus certain differentiæ must be added in order to constitute judgment, and the next section must be taken up with a determination of these differentiæ.

From the genetic point of view there are originally only two kinds of judgments: *existential*, or judgments which assert simple existence, and *relational* judgments, which assert relations among existents. The elements of the existential judgment are: (1) the *objective* representation of something to consciousness; (2) the act of *positing*, which is virtually our willing the ex-

istence or non-existence of the thing. But between the representation and the volitional fiat, let this thing be, there must intervene the motive of the fiat, which is some interest. This interest must coalesce with the representation in order that the volitional pulse may be stirred to utter itself in the let this thing be or not be. Thus arises the simple existential judgment. It is more than mere perception; we must perceive and then do something to our perception before the content or object may exist to and for us. Interest must fall upon the object represented, and there must be that pulse of self-commitment which has been translated into the let this thing be, before the judgment of existence can be said properly to arise. The judgment thus puts a kind of personal stamp of endorsement on the object of perception.

The relational judgment is more complex. Its prototype will be found in volitional alternation, or that process by which the animal or the young child selects out of conflicting, or at least competing means, those which will serve its end. Thus the chick, whose end is food and whose alternatives are cinnabar caterpillars and other caterpillars, will choose the other caterpillars, rejecting the cinnabar species. This process ceases to be purely volitional and takes on judgmental complexion when the alternatives are consciously conceived, or become related in thought as alternative means of satisfying the volitional end; that is, when a body of experience or knowledge becomes the guiding principle of selection. In the chick's case the selective principles are all below the level of *thinking*. The end, food, although not conceived in any intellectual terms, yet functions in the chick's consciousness as a limiting and guiding principle. The chick's universe is one of food, and the included alternatives are food-alternatives. The body of experience acquired by the chick thus conditions its selective activity. The motives of selection rise to the plane of thinking when they themselves become the objects of representation. The child performs a judgment of relation when it pronounces an object good or selects it because it is good. In such an act the relation of the object to some end sought by the child is seized and affirmed. This is the simplest kind of judgment of relation. The more com-

plex forms arise when the less obvious relations on which classifications proceed come into consciousness. In this progress the immediate relations of the object to the survival of the subject gradually drop into the background and the activity takes on a more purely intellectual form. The principal differentiæ of the judgment of relation may then be stated as follows: (1) a body of experience or knowledge which determines the sphere or universe of existential relations; (2) the appearance in this universe of a number of competing alternatives whose relations to some interesting end also rise into consciousness and specifically determine the judgment. These are the differential features of the act in which the volitional pulse of assertion is central and which takes the form of appropriation or rejection of some among the included alternatives.

Now, it may be asked at this point, do we not beg the question when we postulate a universe which includes all the alternatives as a *condition* of the judgment of relation? How else, it may be asked, than through a process of judgment could such a universe arise? We answer that our first universes arise in perceptual experience. Judgment is never without presuppositions. The chick no doubt learns from experience what objects are food for it before it is able to select among the objects presented to it. Our logical universes may be and no doubt are in the later stages of experience, products of logical processes. But this is evidently not the case at the point where judgment first arises. The first universe must be one that is supplied by extra-logical experience. When arrived at, however, the judgment function will operate within it in the manner indicated.

We have then the two distinguishable types of judgment—the Existential, which asserts simply existence, and the Relational, which is more complex and selects among alternatives included in a broader genus or universe of existent relations. If, now, we leave the first species out of view as being for our purposes relatively unimportant, we may say that the judgment function is a *disjunctive operation within a larger genus or universe*. Bosanquet recognizes this character in his doctrine that every judgment involves as its presupposition a larger comprehending judgment. The comprehending term need not,

however, as we have contended above, be a judgment. It may be some body of experience which for the time being functions as the real subject of judgment. In other words, the ground of the disjunction may be purely psychological.

To summarize the discussion up to this point, judgment rises out of volitional gounds. It is informed by the motive of volition and it includes the volitional pulse as its central essence. All judgment then is volitional in its nature. The volitional pulse becomes a pulse of judgment when a field of representation arises to which some interest attaches. The simple judgment of experience is the first result. The subject of a logical judgment need not be logical; it may be purely psychological, a body of extra-logical experience. The judgment of relation preserves the volitional character and simply adds other differentiæ. We have seen that the ground of this judgment is a genus which comprehends, and at the same time limits, the alternatives about which the judgment is pronounced, and that the judgment itself is essentially disjunctive. But the comprehending genus need not be a judgment; it may be psychological and not logical. Now this conclusion is the one that might be anticipated from the point of view of logical immanence.[1] For just as the vital is immanent in the psychic so in the region of the logical processes the later comprehends the earlier which acts as its inner motive and the psychologial is immanent in the logical. We see at this point how the psychological universe, which is the concrete universal of the Lotzean school of logicians, becomes the immanent motive and spring of logical processes, so that it is not necessary to postulate an infinite series of logical universals, but experience passes by insensible gradations from the pre-logical into the logical stage.

The fact that every judgment either affirms or denies led Aristotle to regard affirmation and negation as coördinate moments in judgment. Modern logicians have tended rather to subordinate negation to affirmation, and some have gone so far as virtually to deny the reality of negation. Without delay-

[1] The doctrine of immanence set forth here is not identical with Erdmann's. What it means is the internal activity of psychological content as a motive in logical processes.

ing on historical details, however, we may seek an answer to two questions concerning the negative: (1) How does negation arise? (2) What is its function in judgment? If we bear in mind the relation of judgment to volition we will be ready to agree, I think, that all judgment is positive. There cannot be a judgment in which something is not asserted. All judgment is, therefore, positive and assertative. The distinction between affirmation and denial must then be a distinction between two kinds of positive assertion. So much seems clear. But it is not so clear what an assertion that is neither affirmative nor negative can be or how such assertion can be real. How shall we render the question intelligible? In the first place, it is clear, I think, that when I say in a negative existential judgment, for example, that no griffin exists, the nominal subject, griffin, is not the real subject which motives the assertion. The real subject is something known; some conception of reality which necessitates the denial. Now the assertive force of the judgment lies in the self-conserving force of this backlying knowledge or conception which simply maintains itself against what is incompatible with it. Every existential judgment may then be regarded as the self-assertion of its real subject, *pro* the compatible, *contra* the incompatible. There is always a positive; the self-assertion of the real subject which conditions both affirmation and negation. Suppose, for illustration, that this were not the case and that griffin were the real subject of the judgment, the function of the denial would be to remove its own subject and thus commit logical suicide. The real subject is that which necessitates the denial and is some backlying knowledge or conception of reality which is incompatible with the existence of griffins. The real subject maintains itself against its incompatible. Thus the negative judgment arises. We see, then, how the negative existential judgment rests on position. It is not pure destruction and removal, but something establishes itself in and through it.

If we take the judgment of relation the same fact comes out even more clearly. We have seen that the judgment of relation is disjunctive and that it presupposes a genus or universal that is either logical or psychological. Take the judgment:

men are not infallible. Here the real subject which necessitates the denial is some backlying knowledge or conception of human nature with which the notion of infallible men is incompatible. Let us suppose that this is not the case, and see what follows. The denial simply sweeps away the notion of infallible men and leaves nothing behind. There is thus no motive for further progress. We can escape this irrational result only by identifying the real subject of the judgment with the knowledge or conception of reality that necessitates the denial. This need not be a definite affirmation that men are fallible or even the knowledge of that fact, but rather some knowledge or conception of human nature that is incompatible with its infallibility. This real subject it is that asserts itself in every judgment and renders it positive, whether its form be affirmative or negative. And this it is, and this alone, which enables judgment to make progress through denial as well as through affirmation.

How, then, are affirmation and negation related? The answer cannot be given without recognizing the position of the real subject as the condition and motive of both affirming and denying. Some logicians, as Sigwart and Bradley, take the ground that a negative judgment presupposes an affirmation or an attempted affirmation of the opposite. This is also substantially the view of Benno Erdmann. But it is clear at this point, I think, that what the denial does presuppose is the position, the self-assertion of the real subject. The real subject maintains itself and necessitates the specific denial or affirmation, as the case may be. This real subject is always related to the judgment as the genus or universal within which the affirmation or denial falls. It is this larger assertion, and not a specific affirmation of the thing denied, that is necessarily presupposed in the negative judgment.

In what sense then are affirmation and denial related to each other? We do not inquire here what the actual relation between any two given affirmations and denials may be, for a denial *may* be the contradiction of a previous affirmative assertion; but rather what is the essential and necessary relation between affirmation and denial *as such?* If we bear in mind that it is the real subject that necessitates the judg-

ment which is in its nature, an appropriation of what is compatible or a rejection of what is incompatible with its actual content, it would seem not to be necessary that even proposed or suggested affirmation should precede denial, as is the contention of Bradley and Bosanquet. For if the real subject which is the genus or universe within which the judgment functions, necessitates the affirmation or denial on the ground of compatibility or incompatibility, it would seem to follow that inasmuch as the relation of incompatibility may be directly apprehended, like the inequality of two lines, therefore denial may be direct and unmediated by any suggested or attempted affirmation. It seems gratuitous for us to say that we cannot deny without having first gone through the form of affirming. Limiting the implications of the relation to the requirement of necessity, I cannot see any sufficient justification for the doctrine that negation is mediated by affirmation. On the contrary, so far as the logical relations of the two moments are concerned, they seem to be perfectly coördinate. The real subject approaches the alternatives contained in the limiting genus without logical prepossession and affirms or denies them with direct relation to itself and without regard to their relations to one another.

But from another point of view there is a difference. We have seen that the real subject gets on through both affirmation and denial. But it gets on directly by affirmation, while its progress through denial is only indirect. The organism maintains itself through the avoidance of what is hurtful, as well as by the assimilation of what is beneficial; but the two functions do not advance it in the same way. It is directly benefited by food, but only indirectly and mediately by the avoidance of the hurtful. The same is true of affirmation and denial. While logically they are coördinate in the sense that neither is mediated by the other, yet affirmation ministers more directly to its subject than does negation. Naturally, then, the interest in affirmation will be stronger than that which attaches to denial and, therefore, psychologically, if not logically, negation will be forced into a secondary place.

To the question, then, of the necessary relation of affirmation and negation we answer that logically they are coördinate

and inconvertible modes of assertion, and that the real subject of discourse advances through denial as well as through affirmation. The first part of this conclusion seems, however, to be contradicted by double negation, which by common consent of logicians is held to be identical with affirmation. Now, it is true that the denial of a denial leads up to an affirmative judgment. But this is not the same as to say that double negation and affirmation are identical. The truth is the denial of a denial simply sweeps the first denial away and leaves the ground clean for an affirmation which immediately follows. But this affirmation is a third judgment. That this is true will appear not only from an inspection of the movement of thought in such cases, but also from the consideration that a denial of a denial contradicts it, and leads, therefore, by a process of immediate inference to the assertion of the contradictory affirmative. Double negation is not affirmation, then, but simply prepares the way for affirmation by destroying the negative that blocks its path. It is one of the modes by which the real subject necessitates an affirmative judgment. Logical analysis thus fails to lend any support to the idea that affirmation and denial are not perfectly distinct mental functions, or that there is any point where they tend to lose their difference and become identical.

There has been great difference of opinion among logicians as to whether the negative ought to be referred to the predicate or to the copula of a judgment. If we distinguish at all between predicate and copula, which seems to me to be a doubtful performance, then the predicate will be the name of something that is conceived to affect the subject in some way and the copula will stand for the mode of this affection. The copula may then be regarded as a conceived relation between the subject and the predicate matter. Let us take the judgment, men are not fallible. If the negative belongs to the predicate, then, as Benno Erdmann points out, the judgment becomes affirmative, men are non-fallible and the distinction between affirmation and negation is virtually abolished. But if it belongs to the copula the negative maintains itself and a certain conceived content is rejected by the subject on the ground of incompatibility. Benno Erdmann holds that the negative is to be referred to the copula,[1]

[1] Logik, Erster Band, § 57, 348.

and in this I think he is unquestionably right, and would only take issue with him on the point that what the negative copula sweeps away is an affirmation or a proposed affirmation. It has been shown, I think, that all that is necessarily involved in negation is the presence of an alternative that is equally open to affirmation or negation and that whatever more than this may be involved in any given case must be determined by the context of the judgment. The denial removes an alternative that might have been affirmed had it been compatible, and it removes it as a whole out of the sphere of possibilities. The function of denial is thus always removal, sublation.

The *implications* which the negative judgment may contain is a consideration that is to be carefully separated from that of the meaning of denial. The implications of the denial when it has once performed its function are to be determined in view of the relations of opposition which subsist between it and other conceivable judgments involving the same terms. Denial contradicts affirmation in the sense of wiping it out completely. This insight is as old as Aristotle. But when the denial is taken as a judgment form then it stands related differently to other judgment forms, affirmative and negative. Thus if we say all regular students are eligible to college honors, we, in effect, say that any regular student is eligible. The denial of this implies that there are regular students who are not eligible. This gives the traditional opposition of all are and some are not. But the negative judgment may be denied by others which are not contradictory. Thus all are will be denied by none are. But it is not necessary to enlarge on a topic so familiar. The important point of the discussion here is the necessity for distinguishing between the meaning of denial and the implications of the judgment in which the denial is incorporated.

Benno Erdmann's doctrine of the immanence of the predicate in the logical subject makes it possible for him to speak more profoundly than most logicians on the subject of negation. Every denial, he says, rests on the failure of immanence of the predicate in the subject.[1] This is true. But Erdmann does not, I think, develop the full implication of his own doctrine. If we

[1] Logik, Erster Band, § 57, 353.

take the subject in the narrow technical sense in which it is ordinarily used, then no question of immanence can arise and the denial simply sweeps away a possible synthesis, leaving nothing behind. Thus if we deny that men are infallible we remove the notion of infallible men and leave nothing in its place. Thinking is thus brought to a standstill with no motive for any further progress. In order to avoid such a disaster the real subject of the judgment must be something that survives both affirmation and denial. It must be some universe or piece of knowledge lying in our consciousness which asserts itself in the removal of the incompatible or in the assimilation of the compatible. The real subject survives the denial and gets on by means of it. And it is this subject alone which has immanent in it all the real alternatives on which affirmations might be founded, while denial in such a case indicates a failure of immanence in the sense that what it denies is no real alternative at all. It does not exist within the confines of this larger subject. This amendment I would suggest to Erdmann's doctrine of the immanence of the predicate in the subject; an amendment that would be perfectly consistent with his refusal to allow to denial any independent significance. Denial always, on this view, points back to a larger self-asserting subject, in relation to which it is the cancellation of an affirmative possibility, and although it does not as definitely point forward to affirmation as Erdmann thinks, it does, in fact, prepare the way for the more definite self-assertion of the real subject.

The fruitful question regarding the function and value of the negative is, as Bosanquet says, why in knowledge we cannot do without denial? A full answer to such a question is, perhaps, impossible. But if we have rightly conceived the relation of judgment to the volitional processes which underlie it, an equivalent question would be, why cannot volition do without rejection? The obvious answer here is that the environment contains things that are incompatible with the organism's survival. And just as we have reason to think that pleasure without pain could not supply an adequate stimulus to volitional activity, or a principle of selection that would enable it to avoid the hurtful, so for analogous reasons we have grounds for thinking that knowledge

could not get on with simple affirmation. The infinite sphere of alternatives that may confront any given subject will contain the incompatible as well as the compatible. Now, before the incompatible, affirmation is powerless. There is needed a selective principle which will enable the subject to assert itself against and in spite of the incompatible. Hence the necessary function of negation. Knowledge makes progress as much by denial as by affirmation. But it progresses in a different way through denial, and at no point can the two modes be identified.

Denial we have seen to be a selective principle in the activity of knowing. In practice, however, it possesses various degrees of selective value. To begin at the bottom of the scale the value of negation is at its minimum in what Kant has called the infinite judgment. This judgment definitely assigns the negative to the predicate of the judgment. When we say, for example, virtue is not four-cornered, we assign virtue to the infinite universe of non-four-cornered things where it has stones, vegetables, caterpillars and other things for its companions. The negative is at its lowest terms here because it is most indeterminate; it has simply expelled virtue from the province of four-cornered things, but otherwise leaves it to wander at large in an undetermined universe. If we leave out of view the infinite judgment and connect denial where it properly belongs, with the copula of the judgment, its value will be found to vary indefinitely. Its function is uniform, the removal of a false alternative, but what this removal does for knowledge is variable. The point on which I wish to put emphasis in this connection is that the significance of negation will, other things being equal, vary with the extent and richness of the real subject, which necessitates the denial. The denial of the scientist means more for knowledge than that of the unlearned, though both denials be equally valid. The denial of the child is less significant than that of the man. The savage looks out on the stars and shakes his head; the trained astronomer, looking through his telescope, makes the same sign. The difference in significance is vast, and why? Because the denial is necessitated by knowledge, and just in proportion as this knowledge is rich and exact will the denial be definite and specific. The astronomer's denial, perhaps, brushes aside a

false hypothesis, or removes the only obstacle in the way of a great discovery, while that of the savage signifies, it may be, only the failure of some combination which has a superstitious import to his mind. Some of the later writers on logic represent this tendency of negation to become more specific as approximation to the significance of affirmation. Or, to put the same thing in different language, denial tends to become the equivalent of affirmation until at the highest point it has the same value. It seems to me, however, that these logicians state a truth in language that is misleading. In order to say anything intelligent about the value of denial we must first distinguish between its function and its implications. The function of denial is always and invariably removal. As such it is as unique in its character as affirmation. We have also seen that in its relation to the knowing process it is a principle of selection. In this regard it is also unique, and not to be merged in affirmation. But the implication of denial will in most instances, at least, be something positive. It will at least limit and define the sphere of alternatives by removing the false and incompatible. And as knowledge becomes richer and more specific a denial will come to point with greater and greater precision to an affirmation which will be involved in it by some relation of opposition. It is incident on the growth of knowledge that the system which it immanates becomes more closely knitted together and that the judgment functions become more specific. If knowledge could once complete itself we would then have a subject whose every affirmation would exclude a specific negation and whose every denial would lead by direct implication to a specific opposite affirmation.

CONTRIBUTIONS FROM THE HARVARD PSYCHOLOGICAL LABORATORY.

COMMUNICATED BY WILLIAM JAMES.

A. Discrimination in Cutaneous Sensations.

By Leon M. Solomons.

It is well known that the power to discriminate between a touch from two points and that produced by one is susceptible of great improvement by practice. But the process by which this comes about, as well as its general bearing on the origin of cutaneous perceptions, has been considered only speculatively. At the suggestion of Professor James I undertook an experimental investigation of this problem.

My first object was to determine whether it was a simple question of exercise, like the growth of a muscle through use, or whether there was a distinctly mental element of an educational nature, allied to such processes as learning to read. For this purpose two subjects were selected and each regularly practiced in the discrimination of touches made with dull compass points on the fleshy part of the forearm. But one, S, was regularly told whether he was right or wrong, while the other, G, never was. At the start both distinguished two points as two at about a distance of an inch and a-half. After a few weeks' practice the one who had been told when he was right and when wrong, S, had reduced the distance to about one-fourth inch, while the sensitivity of the other, G, remained practically the same. To make sure that this was not due to individual differences the second subject was then for a while told when he was right and when wrong, whereupon he improved rapidly. The subjects were always touched with one point about as often as with two, care being taken to avoid any kind of regularity in the alternation. The tendency to call one two was often as marked as the tendency to call two one.

From these experiments I concluded that the process of training was essentially a mental one, a real learning, a formation of new associations.

The next question then was, what peculiarity of the sensation produced by two points causes them to be recognized as such. This seemed to resolve itself into two lines of inquiry:

1. What other judgments as of distance, area, locality, etc., seem to be related to this?

2. How must the sensations from two points, and the conditions of their application, be varied in order to have them apperceived as one and similarly, *mutatis mutandis*, for one point?

Along the first line experiment soon developed the following:

(*a*) The impression of area-covered which accompanies a judgment of one, is often greater than that accompanying a judgment of two in the same neighborhood; so we cannot suppose the judgment of twoness to be based upon the impression of area. In other words, when we judge the points to be double it is not because they seem to affect us over more than a certain area, which area we take to characterize the contact of a single point.

(*b*) In a subject trained to discriminate two points from one, the ability to *localize* touches, that is, to touch with the other hand the place touched, was no better than the average, and not nearly so accurate as the discrimination. That is, the error in localizing, measured by distance, was much greater than the distance apart necessary for discerning two points to be two. From this it would appear that judgment of doubleness does not depend upon separate localization of the different points; that we do not know the touch to be caused by two points by perceiving them as in two different locations.

(*c*) When touched in one place and then in two others, an untrained subject's ability to tell which of the second touches was nearer the first, was much more accurate than his ability to detect simultaneous doubleness, measuring accuracy by distance as before. A difference of less than one-half inch between the distance of the two *successive* touches from the first was readily perceived by a subject whose discrimination of *simultaneous* doubleness required a distance of one and one-

half inches. This would seem to shut out any theories trying to explain the detection of simultaneous doubleness through a feeling of 'distance-apart.'

Along the other line of inquiry, the first thing tried was expectant attention. The subject was told beforehand what the stimulus would be, but requested to make judgment entirely independently of this knowledge, so that the effect of the suggestion upon the actual feeling might be judged. The result was that two points were felt as two when so near together that without the expectation they would certainly be perceived as one. The judgment was fully twice as delicate when aided in this way.

Then the subject was told that he might be deceived—that when told that the stimulus would be two, it might really be one. He was to put himself in the condition of expectation for the stimulus as it was told him it would be, but to take care he was not deceived when it came to judging. The result was always as in the previous experiment. That is, the influence of the expectation predominated, so that when touched by one point he would perceive two if he had been led to expect two; and when touched by two, set farther apart than was necessary for perceiving them as two ordinarily, he would perceive them as one if told to expect one.

Judgments of two might often be changed into judgments of one by inserting a dull pencil point between the compass knobs. Similarly a judgment of one could often be changed into two by touching the arm in some other place. The extra touch seeming to suggest the doubleness of the stimulus without itself being clearly perceived or localized.

The absence of connection between the judgment of twoness and other judgments—area, position, etc.—as well as its great susceptibility to suggestion, both direct and indirect, seemed to point to the whole thing being nothing but a matter of simple association. We learn that a certain kind of sensation means two points, just as we learn that certain marks mean the letter H, that another group of sensations means 'book,' etc. If this were true it ought to be possible to establish any arbitrary association desired—to train a person so that he would call one two

and two one. To test this the following experiment was tried: Beginning with the points far enough apart to be readily perceived as two, the subject is practiced in discriminating this sort of contact from that of one point. The touch from two points is made by a rather sharp blow, and in one region of the arm; while that from one is made more by a pressure, and in an adjoining portion of the arm. Gradually the double points are brought nearer together. The moment the subject shows a tendency to call them one they must be separated farther again and approached more gradually. When the two points have in this way been brought very near—I always waited until they were within one-fourth inch of each other—the position and character of the touches are reversed. That is, the double points are now *pressed* down, and in the place where the single point was formerly applied, while the single touch is made with a blow, and in the place where at the start the double touch was made. Under these circumstances the judgment reverses, two is called one, and one two. That is, the peculiarities of the sensation due to the method of application, and the locality, have completely superseded those due to the number of points, as a basis for the judgment. Generalizing, we might say: any cutaneous sensation may give rise to a perception of two contacts, if the past experience of the individual has established the proper associations. That we commonly do not make errors in this regard is due to favorable past experience. An artificial environment might educate us entirely otherwise.

Coming back to our original question—the process by which improvement comes about, and its general bearing on the theory of cutaneous sensations—we may say that it cannot be traced to a refinement of our power to localize, or to a refinement of, or an establishment of relations with, our judgments of distance or area. Simple direct association between the sensation produced by two points, and the idea of two points has been shown to be perfectly capable of explaining the phenomena. That it actually is the cause is a conclusion that seems almost forced upon us. But this particular judgment is so intimately connected with other cutaneous judgments—position, area, etc.—so probable is it that

what holds for one holds also for the others, that it would seem well to postpone a verdict until they have been similarly investigated and shown to obey similar laws. This I hope to be able to show in the near future. Meanwhile we may give a provisional answer, at least, to the second of our questions. Since any cutaneous sensation may be judged two if the proper associations are established, then our *perception* of *two-touches*, even though obtained through sensations of touch only, must involve other elements. The elements of number and space which enter into the complete presentation must be non-cutaneous, that is, associated with the activity of a different portion of the brain from that immediately connected with the nerves from the skin.

B. Studies in Sensation and Judgment.[1]

By Edgar A. Singer, Jr., Assistant.

I. differentiation of sense organs.

A group of coördinated experiments was conducted with the view of determining how far the process of differentiating the physiological bases of sensation could be carried. These included the sensory fields of touch, pain, temperature, taste and sight. The limits of the present contribution compel us to confine ourselves to the first three of these fields as perhaps the most interesting.

a. Touch and Pain.

The results of Goldscheider, Frey, Nagel, Dessoir and others have given a decided impetus to the investigation of the peripheral apparatus connected with the senses of touch and pain.

[1] This and the following studies are the partial outcome of a course given to the undergraduates of Harvard University during the Winter and Spring of 1896. In permitting them to be published I am conscious of the comparatively elementary character of the work and of the little that it offers in the way of real conclusions. Nevertheless, it occasionally presents new points of view, and, where this is not the case, the care with which the experiments were performed renders them, I think, worthy of consideration as confirmatory evidence in fields where such is greatly needed.

E. A. S., Jr.

With these results before us, a series of experiments was instituted with a view to the answering of the following questions: 1. Can points be found possessing consistently different thresholds of touch, and, if so, how great a difference of threshold is to be found? 2. A precisely similar question with respect to pain. 3. Do points bearing similar threshold relations for touch and pain coincide? For example, is a point sensitive to touch also sensitive to pain, and so on? 4. Can the threshold values of touch and pain be made to vary independently? 5. Are there any constant conditions of distribution?

Two groups of experiments were conducted: the first rather preliminary in nature and broader in scope, but accurate enough to deserve mention; the second conducted with greater refinement and confining itself to the pain-sense. The apparatus in the two series was the same, to wit: a delicately poised balance beam, on one end of which was fastened sometimes a bristle, sometimes a fine jeweler's needle. The arcs through which the beam swung were marked upon a graduated scale. By displacing the beam through a constant arc, releasing it and allowing it to swing freely toward a position of equilibrium, very constant conditions of applying the stimulus were obtained. The portion of the body operated upon (the back of the hand, or the volar surface of the arm) was laid in a clay cast and held firmly in such a position that the end of the bristle or the point of the needle would just make contact with it when the balance was in equilibrium.

In the preliminary series above mentioned a small portion of the skin (a rhomb of 4 x 6 mm. about) was selected on the back of the hand. In this region eleven spots were located by minute peculiarities of the skin, for the most part revealed only by the use of a lens. No ink was used in marking these points on the hand: the spots were plotted on paper and designated by numbers. This region was gone over in two ways: first with a bristle and then with a jeweler's needle. Each point was stimulated from fifteen to twenty times in each way; the experiments extending over a number of days. The series was completed with one subject only, although the experiments performed on other subjects gave no cause to

doubt our main conclusions. When the bristle was used, various arcs of fall were first tried until one was found (2° 30′) which sometimes yielded a sensation, sometimes remained unfelt. When the needle was used, several arcs (10°, 7°, 5°) fulfilled these conditions; and all were used, the average result of the three being taken. The results obtained may be graphically represented as in Fig. 1. The numbers on the horizontal line

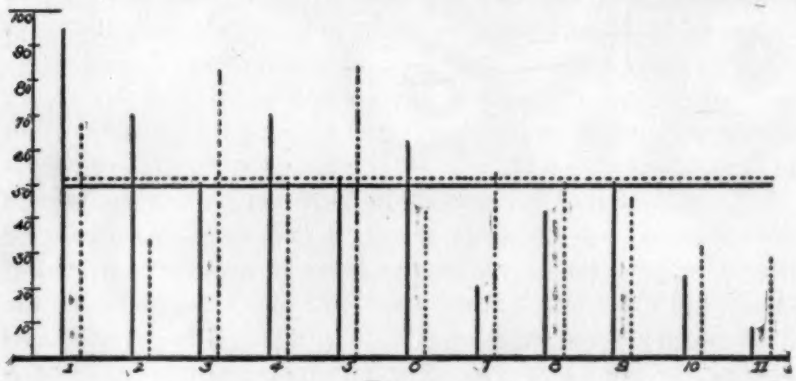

FIG. 1.

stand for the eleven points selected for investigation. Of the two lines corresponding to each point the full line represents the percentage of times the application of the bristle yielded a sensation of touch; the dotted line, the percentage of times stimulation by the needle yielded pain. The horizontal full line represents the average percentage of times the bristle yielded a sensation at the points touched; the horizontal dotted line, the average percentage of times the needle produced pain at the same points. We may take these percentages as measures of the sensitiveness of the points to touch and to pain stimuli respectively. The threshold of each kind of sensation and the mean of these are only capable of comparison *inter se*. All that we wish to determine is whether points possessing more than average sensitiveness to touch coincide with those possessing more than average sensitiveness to pain and *vice versa*.

It is exceedingly difficult to obtain consistent results when we vary the conditions of time as they should be varied, keeping only the points touched as nearly as possible constant. The lat-

ter presents a most difficult problem, and is, probably, the chief source of variation. The results, therefore, must be accepted with all the reserve that is naturally inspired by an average of considerably varying elements. Nevertheless, until we obtain more satisfactory evidence, the following view of the problem may be conservative enough, and within the limits of observation. To our first two questions we should say that differences of threshold of pain and touch respectively do exist corresponding to fixed points of the skin, and that these differences are considerable enough to be easily noticeable. To our third question we should answer: the minimal and maximal thresholds of touch and of pain respectively cannot be readily shown to coincide locally and probably do not coincide.[1]

As the most general formula for the results so far obtained we might say: the sensitiveness to touch and the sensitiveness to pain are not identical functions of position. Our fourth question raises the query as to whether the sensitiveness to touch and to pain respectively are similar functions of some other variable. One variable factor that at once suggests itself is the condition of the skin. The variation of this factor with the location of the point touched (*e. g.*, location in a furrow, or on a ridge), we shall discuss later. But it can be made to vary artificially for the whole region by softening the skin with warm water, soap and glycerine. Under these conditions the threshold for touch was quite noticeably raised, though apparently not for all points. The few experiments tried on the threshold of pain under these conditions showed it to be lowered. A more complete research in this direction is needed; but these results were fairly marked. If they are correct we could answer our fourth question in the affirmative. The sensitiveness to touch and to pain respectively can be made to vary independently; the sensitiveness to touch varying inversely, that to pain directly with the softness of the skin covering the parts affected.

[1] An exception should be made in the case of the point marked 1. This point was sensitive both to touch and to pain. It was, however, a singular point, lying at the base of the longest hair of the region and at the junction of several furrows. It could be compared with points described by Goldscheider and interpreted by him as being the seat of a number of close-lying points of different specific nature, such as at other locations we find separated.

In the second series of experiments (conducted by Messrs. Kline and Parker) a refinement was introduced in the method of mapping the surface experimented upon. To this the 'buttered' side of a glass slide covered with melted paraffine was applied. The paraffine froze upon striking the skin, and not the slightest furrow escaped the paraffine 'print.' The print was then transferred to paper by means of a pantograph, thus being enlarged to any convenient size. The general method of experimenting was the same as before, only the jeweler's needle being used and only the threshold of pain investigated. We need therefore only describe the conclusions reached after a large number of experiments.

1. After increasing the intensity of the stimulus beyond the threshold of touch, one of two results was obtained. Either (a) the increased intensity gave only touch even after blood was drawn, or (b) the touch sensation merged into pain.

2. Points exceedingly sensitive to pain gave this sensation with an intensity of stimulus very slightly above that which marked the threshold of touch.

3. The points sensitive to pain were distributed in a characteristic way. Calling those points that gave only touch with considerable intensities of stimulus (corresponding to areas of from 15°–22°) non-sensitive, and those points that gave pain for low intensities of stimulus (5°–10°) sensitive points, the following facts of distribution were revealed: 97% of non-sensitive points were on the elevations of the part investigated, 3% were in the furrows; 73.5% of the sensitive points were in the furrows, 26.5 were on the elevations. Thus it will be seen that sensitive points are more numerous in the furrows of the skin; non-sensitive points mostly confined to the elevations.

To these definitely objective results may be added those which depend upon the introspection of the subject. In this way the following points were brought out:

1. In the field of touch it is perfectly possible to distinguish between ordinary touch and pressure (the 'kernel-like' feeling of Goldscheider). We did not succeed in obtaining consistent results showing these sensations to be permanently attached to definite points, but it is not improbable that a more extended

research would prove them to be so. However that may be, the difference was only such as could, in the opinion of the subjects, be explained by the structure of the skin at the point affected.

2. The qualities of pain produced at sensitive points were quite different. They were called 'acute,' 'aching,' 'numbing,' 'pricking,' 'tingling.' These different qualities were fairly characteristic of the point touched and did not replace each other to any great extent.

3. Nearly every pain was preceded, or succeeded, or both, by an itching or tingling not to be found in touch points. Sometimes the tingling would remain alone without pain, but usually at a point that was in the habit of yielding pain.

4. Pain could thus follow touch or tingling after a considerable interval, but when it was of the 'acute' kind seemed to be just as immediate as touch.

No theory of the physiological basis of touch and pain could be deduced from such results as the preceding alone. Such a theory must, we feel, take into account a much larger range of facts, notably those connected with the independent variation, under pathological conditions, of the touch and pain senses. It may be well, however, to point out a few theoretical inferences from our experiments: 1. From the fact that the threshold of touch is raised for some points by softening of the skin, while that of the pain points is lowered; from the fact that pain points are more numerous in the furrows where the skin is softer; and, if Goldscheider's observation is true, from the fact that touch points are more numerous on the elevations where the skin is harder; it would seem that touch is dependent, not only on the nervous equipment of the skin at any point, but also upon its ability to conduct the stimulus to surrounding points. Thus touch might quite well be possible at a point from which pain (under proper conditions) was absent, without showing that a nerve could be sensitive to touch that was not sensitive to pain. 2. On the other hand, any theory that tried to identify the nerves of touch and of pain would have to show that the points of greatest sensitiveness to the one sensation were also unusually sensitive to the other. For while the fact that conditions of the skin which

render it a better conductor of stimuli may increase the sensitiveness to touch of certain points not provided with nerve endings (much as a small coin placed on the skin would transmit a slight pressure better than would a bit of dough under the same conditions), yet it does not seem plausible merely to identify touch points with points of best conduction. It would not, for example, seem probable that a point supposed, on the ground that it was unusually sensitive to pain, to be provided with an easily stimulated nerve-ending, would be less sensitive to touch than some hard part of the skin which yields touch by transmitting the stimulus to several nerve-endings. It would seem to require greater energy to set all this machinery in motion than to affect one slightly protected nerve-ending.

So far as our experiments go, then, we regard them as pointing toward a discreteness of the end apparatus of touch and pain. The explanation of the different 'timbres' of pains (their 'acute,' 'tingling,' 'aching' etc. character,) would require a much more general discussion than would be here in place. We can only suspect that we are here dealing with a more complex sensation than mere touch or mere pain, but that it is not yet necessary to suppose a specific apparatus to explain these peculiarities of sensation.

b. Temperature-sense.

The now well-known results of Goldscheider[1] formed the basis of a series of experiments on the temperature sense. While the grosser results of this investigator are now largely accepted, those who have tried to confirm his more refined (and perhaps more important) conclusions have realized the difficulty attending such an effort—a difficulty that has been recently expressed as an impossibility by Dessoir.[2] In such a field the testimony of many observers is needed.

After trying various kinds of apparatus the original metal cylinders of Goldscheider were found to afford the most satisfactory means of applying the stimulus. These cylinders were heated in water at 36° C. for locating warm spots, and cooled

[1] Goldscheider, Archiv f. Physiologie, '89.
[2] Dessoir, Archiv f. Physiologie, '92.

in a similar way to 3–4° C. for locating the cold spots. The surfaces investigated were on the back of the hand and on the volar surface of the arm.

The investigators (Messrs. Marsh and Mathews) found the arrangement of the spots as Goldscheider has described it. The spots radiate from centres at which they are relatively numerous. The centres of the cold spots usually lie close to, or coincide with, the centres of the warm spots. The cold spots are, on the whole, more numerous, they react more quickly and are more easily located. Again, both cold and warm spots seem to vary *inter se* in the strength of their reactions. Their sensitiveness differs at different times.

The investigators regard as the chief interest of their experiment the results of applying mechanical and electrical stimuli to the spots already located by a temperature stimulus. The mechanical stimuli used were the metal cylinder before described, but kept at a mean temperature, and a slender wooden splinter, with a cork tip of the same size as the blunt point of the metal cylinder. By a slight pressure upon the temperature spots already located (and marked in Goldscheider's way with dilute ink), warmth and cold were experienced at the warm and cold spots respectively. The same success attended the experiments with electrical stimulation. The stimulus was applied by means of an electric needle, the point of which was brought lightly in contact with the temperature spot; a weak current, just failing to give the usual tickling sensation, being employed. Both mechanical and electrical stimuli were tried only on the spots that had before proved most responsive. In every case, however, in which they were tried, they succeeded.

Finally, the analgesic nature of the temperature-spots was investigated. In every case one point within the spot was found which would bear a heavy weight on a stimulating needle without yielding pain. The needle point being, of course, much smaller than the spot of ink marking the temperature spot, it was necessary to experiment within this area for some time before an analgesic point was found. In no case, however, did the investigators fail to find such a point.

As a result of their experiments the investigators feel that

Goldscheider's results are generally reproducible. It is beyond the scope of the present communication to comment upon his deductions from these data.[1]

II. Intensity.

With the advance of psychology more and more attention is being given to questions relating to the nature of Intensity of sensations. Not only have the meanings of 'threshold' and of 'difference threshold' frequently been discussed (of which later), but the nature of the concept of intensity itself has been treated in new ways (*e. g.*, by Münsterberg). Experimental psychology has so far had little to do in the discussion, which has remained theoretical in character. It is possible, however, that the laboratory, by investigating the conditions upon which variations in the judgment of intensity depend, may contribute something to the settlement of the problem.

The facts that find expression in some such law as that of Weber, whatever may be their ultimate meaning, at least reveal this: that between the point at which a stimulus is administered, and that at which a judgment of its intensity is pronounced, a certain factor enters into the result which, for brevity's sake, we may call 'subjective.' The search for this subjective factor has largely confined itself to imagining ways in which the energy represented by the physical stimulus might be lost before setting in motion the apparatus of judgment. It seems plausible, however, to suppose that the physiological effect of every stimulation is not merely to excite some special sensory nerve, but to produce a profound change in the entire organism, and, further, that the judgment of the nature of the stimulus must have for its physiological basis these secondary effects of stimulation as well as the primary one of the specific sensory excitation. It is impossible to say, *a priori*, how fully such secondary effects as these represent that which we have above called the 'subjective'

[1]One must always hesitate to say that suggestion has played no part in such experiments as those here described. A subject sufficiently trained to be a reliable observer of the delicate phenomena involved must also have become familiar with the results of past workers in the field. We can only say that, so far as a conscientious effort to be unbiassed effects freedom from suggestibility, the experiments here recorded are as reliable as can well be hoped for.

factor in the judgment of intensity. It cannot be without interest, then, to examine the way in which variations of the physiological reaction to a stimulus affect the judgment of the intensity of the stimulus. The reaction must, of course, be a very general and a very subtle one. Nevertheless, we may take simple types of it in the form of pronounced and readily measurable reflexes. A series of experiments along this line we shall proceed to describe.

Judgment of Intensity as Affected by Involuntary Reactions to Stimulus.

In this series, conducted by Messrs. Dearborn and Gaylord, use was made of the well-known phenomenon of the knee-jerk. The apparatus, being similar to that used by all experiments in this line, needs no description. The jerk in the vertical plane was selected as being most convenient for the purpose. About 300 experiments were made, each consisting of a pair of stimuli, one being kept constant, the other being made equal to, greater or less than this normal. The subject was directed to estimate the intensity of the second stimulus with respect to the first. The experiments were made upon three subjects, one of whom was throughout ignorant of the object of the experiment. Sometimes the squeezing of a hand dynamometer was found useful for increasing the general innervation. The specific object of the investigation was to find how far the relative lengths of the jerks accompanying the two stimuli determined a subject's judgment of the relative intensities of the blows.

In examining the results there are two cases to be considered. Either (1) the relation of the second kick to the first was the same as the relation of the second stimulus to the first, or (2) the relation of the second kick to the first was different from that of the second stimulus to the first.

Examining now the distribution of the right and the wrong judgments, the following facts are revealed: (1) Of the right judgments, 71% were in cases in which the relations between the stimuli were the same as the relations between the kicks; in 29% they were different. (2) Of the wrong judgments, in the cases in which the relations between the stimuli were different

from the relations between the kicks, 93% were in accord with the relations between the kicks, only 7% were contrary thereto. In brief, then, an agreement between the relations of the stimuli and those of the kicks increases the percentage of right judgments; in the case of a disagreement between the two the judgments, under the conditions of the experiment, showed a far greater tendency to be determined by the relations of the kicks than by those of the stimuli.

There are three possible interpretations of these results. Either (1) the physiological conditions that favor a greater kick include the conditions of a heightened sensibility, or (2) the association between a larger reaction and a more intense stimulus being established, the influence of the kick upon the judgment of intensity involves a more or less direct inference from our psychological experience, or (3) our concept of intensity contains as an immediate element, the muscular sensations arising from our adaptation or reaction to the stimulus.

The first explanation finds nothing to support it in what little we know of the physiology of the knee-jerk. While it is not at all an impossible hypothesis, it is not a necessary one, and confirmation of it can scarcely be hoped for. The second hypothesis is a perfectly possible explanation, and finds analogies in many of our psychological experiences. For example, when a large object of the same weight as a small one is judged to be heavier we have the influence of an association of this kind. The third hypothesis must exist for the present as a mere speculation, and our attitude toward the phenomenon will depend, for the present, largely upon the theoretical stand we take respecting the nature of intensity in general. It can only be said that the phenomenon would be of the nature we should expect if one of the chief 'subjective' factors in our judgment of intensity were the reaction of the organism to the stimulus; for here we find the variations of that reaction to be the chief determinant of variations of the judgments.

III. Judgment.

There is, perhaps, no problem calculated to throw more light upon the psychology of judgment than that of the nature of a

difference threshold. Attempts to fix the meaning of the term have robbed it of that confidence reposed in it by the older psycho-physicists. The essentially relative nature of the concept has been insisted upon by Wundt[1]; in a still more thorough-going way by Fullerton and Cattell[2] and a mean position, seeking to define in more careful terms the meaning of the much used term 'just noticeable' stimulus or difference of stimuli has been adopted by Schumann[3]. We have found it helpful to treat the judgment simply as a reaction. The organism is affected by a certain kind of stimulus. We find the organism reacts in several ways to the stimulus, one of which ways may be the expression of a judgment. Between these two objective facts lies a chain of events within the organism of which we know little, but whose nature we may be led to suspect by introducing new factors and noting the variations resulting. One form of variations concerns the time elapsing between stimulus and the reaction, which reaction may express, in some form or other, a judgment. Another variation concerns the amount of stimulus that is required to produce a given kind of judgment. So far we are not dealing with theory, but simply trying to obtain a general expression of the facts of the case.

Our work was confined to the judgment of differences. This is only an apparent limitation, since all judgments are judgments of difference. Whether we say that there is a difference in intensity between two tones presented to us, or that a book is before us, we are in both cases dealing with judgments of difference. Only, in the latter case the judgment is more complex, involves more comparisons, some of them with that which is not immediately given. Turning then to the problem of the judgment of difference as the most general that we can consider, the question arises as to the nature of a threshold. Evidently our only objective criterion for the fact that a difference is perceived, or rather what we mean by a difference being perceived, is that the subject reacts to it correctly. Thus, he says that a difference is be-

[1] Wundt, Grundzüge. 4 te Auf. V. 1, p. 397.

[2] Fullerton and Cattell, Perceptions of Small Differences, p 11.

[3] Schumann, Zeitschrift f. Physiologie u. Psychologie d. Sinnesorgane. V. 6, p. 476.

fore him, or he imitates the difference in some way, or he indicates a difference a certain percentage of the times that one is presented. Now it does not follow that all these kinds of reaction are 'set off,' or discharged, by the same degree of difference between the stimuli. To say that a subject reacts rightly, although he does not perceive the difference presented, means that one kind of reaction is set off by less difference than is another (the vocal expression of a judgment, for example). The various ways in which the judgment is expressed give in turn, as is known, different values for the threshold.[1]

Just such variations as these are significant. It would be a wrong conception of the nature of a threshold to suppose them to be accidental errors to be eliminated, or to suppose that any one result came more near to being some 'real' threshold value than any other. The discrepancy arises from the fact that what we call a judgment of difference is not of the same psychological nature in each case. If we analyze the intra-organic elements that enter into a judgment-reaction we may roughly divide them into the centripetal (sense organs, directly or sympathetically involved, conducting tract, etc.); the centrifugal (*e. g.*, motor coördination involved in expressing a judgment); and the central (the perceptive background upon which the stimulus falls). The first two factors interest us only in so far as we must keep them constant while examining the third. Our problem is: How may the central apparatus of judgment be varied?

a. Preperception.

An element that has been found to affect the quickness of our reaction to a stimulus suggests itself as probably having an influence upon the sensitiveness of this reaction. This element it is convenient to call *preperception*, *i. e.*, an expectation, the psychological elements of which we need not stop to analyze, of the kind of stimulus that is to be presented. Experiments involving this factor can be arranged in a great variety of ways. We confined ourselves to a very simple and, therefore, a very difficult case. Ordinary series of experiments for the purpose

[1] Cf. the extensive work of Merkel on psycho-physical methods. Wundt's Phil. Stud.

of determining the difference-thresholds of intensity and of pitch of sound were instituted. In a number of successive sets the kinds of difference presented, and hence the possible kinds of judgment, were increased in variety. There were four types of experiments including:

I. Difference of intensity in one direction; *i. e.*, the subject knew that the test stimulus would be always greater (or always less) than the normal.

II. Difference of intensity in two directions (*i. e.*, greater and less).

III. Difference of pitch in two directions (*i. e.*, higher and lower).

IV. Difference of intensity and difference of pitch, each in two directions.

It will be seen that in I there was only one possible judgment of difference. In II and III there were two; in IV there were four possible judgments of difference. The subject was always asked to compare the two stimuli in all the respects in which they could differ. The apparatus used was a tuning fork (Ut_4) the prong of which was struck by a rubber hammer falling through a vertical arc. The intensity was varied by changing the arc, while the pitch was altered 2 or 4 vibrations per second by placing a piece of wax on the prong. The experiments were performed on two subjects, whose results we shall give together, since they are of the same kind. In each series the similar stimuli and each kind of difference were given in equal numbers. The number of experiments was not equal in all the series, the lowest number being 250 in series III. The following table gives the percentage of right judgments of sameness and difference in each series:

	Intensity.		Number
	Same.	Different.	of Possibilities.
I.	71.5	57.5	1
II.	61.0	56.3	2
IV.	41.3	44.5	4
	Pitch.		Number
	Same.	Different.	of Possibilities.
III.	79.0	49.8	2
IV.	71.8	32.5	4

From this table it will be seen that the greater the number of possibilities of judgment the less the accuracy of the judgment or the higher the threshold. To present this as a set formula we might say: the sensitiveness to differences between stimuli varies inversely with the number of possibilities of judgment presented. A parallel formula has already been stated connecting the quickness of discriminating reactions with the numbers of possibilities presented. And the two would seem to have similar theoretical explanations. Every perception involves a combination with the stimulus of a concept prepared by past experience. A stimulus is above the threshold that is sufficiently strong to set in motion the central mechanism that corresponds to this concept. Usually the number of concepts likely to be aroused by any stimulus is only limited by the context of events into which the stimulation has entered: in the experiment we still further limit this number. In proportion as we limit it do we lower the threshold value of the stimulus. In our present experiment it is, of course, with the difference between stimuli, each of which is perfectly noticeable, that we are dealing. The concept in question, then, is that of a kind of difference, and it is the number of these concepts likely to be awakened that we limit. But as before stated, the fact that the judgment is given as a judgment of difference does not change the nature of the problem.

b. General and Specific Judgments.

The last experiment, showing the effect of the psychological factor of preparation, leads to a new question. However great the number of differences presented in such a series as the preceding, *i. e.*, however numerous the possibilities that offer themselves to a subject at every choice, there yet remain two factors constant throughout, to wit: sameness and difference. The question arises whether, in case we could in some way obtain judgments that correspond to difference in general and compare with these judgments of particular kinds of difference obtained under the same conditions of stimulations, we should find the two classes to possess different threshold values. It was with this question that we sought to deal.

A few observations drawn from every-day life may be appropriate by way of introduction. It is well known that where differences exist, comparatively slight in nature, between objects of considerable complexity of structure (such as human faces) one may be quite aware of a difference without being able to tell in what the difference consists. And this is not merely owing to our lack of a name for this particular kind of difference, for we may afterwards recognize it in the color of the eyes, the size of a feature, etc.,—differences that are easily describable. Have we here a mere anomaly, or a phenomenon deeply pervading our mental life?

Again, one may quite frequently, when expecting a certain kind of difference, react to another kind. The reaction is right in so far as there is some kind of difference presented; it is wrong in judging what that kind is. A series of experiments will illustrate and confirm this statement. With a small dynamometer provided with a blunt point, the skin on the volar side of the arm was pressed. The series of 250 experiments upon a subject unacquainted with the object of the experiment included 100 in which there was no difference between a normal stimulus and a second stimulus which the subject was asked to compare with it; 75 in which there was a slight difference of pressure, and 75 in which the same pressure was administered at a slightly different point. Both of these differences were below what is ordinarily called the threshold. The subject was informed that the experiment was designed to determine the threshold of difference of location, and that the stimuli would sometimes be the same and would sometimes differ in location. The judgments were always given as same or different in location. The following table gives the percentages of judgments of sameness and difference:—

		Judgments.	
		Same.	Different in Location.
Stimuli.	Same.	69.	31.
	Dif. in Location.	40.	60.
	Dif. in Pressure.	57.	43.

There were then 43 % of the stimuli that did not differ in location, but differed slightly in pressure, that were judged to

be different in location. But so were there 31 % of stimuli that differed in neither that yet were judged to be different in location. The difference between these, viz. 12 %, represents the percentage of times in which the subject was led by the presence of an unexpected difference to judge that an expected one was presented. This judgment was right in its general, wrong in its particular character. Examining the subject afterward revealed the fact that she was ignorant of any differences, save those of location, having been present.

The problem of the relation between the thresholds of general and of specific judgments of difference may be approached in several ways. The simplest way that suggests itself would be to conduct two series of experiments,—in one of which the subject would be asked to judge whether or not a difference were present, in the other of which he would be required to pronounce upon the kind of difference; several different kinds being included in each case. This would, of course, presuppose an ability on the part of the subject to distinguish consistently between the awareness of difference and the awareness of a special kind of difference. It must be confessed at the outset that the absence of such an ability would by no means settle the question. Still, since it is impossible to pronounce upon such a question *a priori*, a series of experiments was conducted with the view of seeing how far such a distinction between general and specific judgments was possible.

The experiments were conducted by Messrs. Holt and Southard, the apparatus used being the same tuning fork struck by a falling hammer that was before described. Differences of intensity were obtained, as before, by varying the arc of fall, differences of pitch by placing a small piece of wax at different heights upon the prong of the fork. The subject was asked to judge between the stimuli varying in intensity and in pitch. There were two subjects; upon each of which were performed 400 experiments, arranged to eliminate the effects of practice and of over-estimation. They were divided into two sets,—in the first of which the subject was asked to decide simply whether the stimuli were alike or different; in the second of which he was required to pronounce upon the specific differences presented

(*i. e.*, greater or less intensity, higher or lower pitch, in any combination). Calling the first set general judgments, the second specific, the results may be classified as follows, the figures giving the percentages of correct judgments.

Subject.	General.	Specific.
S.	70.0	74.0
H.	74.4	82.7
Mean.	72.2	78.4

It would seem from these results that, so far as a voluntary attempt to distinguish between the general and the specific elements in judgment is concerned, the advantage of sensitiveness is somewhat in favor of the latter. The introspective testimony of the subjects may suggest a reason for this. "It would seem to both subjects that to look for general differences and not to perceive specific was as easy as looking for a star with the eyes voluntarily closed. The subjects could not voluntarily assume the attitude of mind for perceiving general differences. The subject asked to make general judgments really made specific. All he could do to assume the 'attitude' was to think vaguely of nothing and to relax his attention. Thus the less accuracy of the general judgment as compared (in the above table) with the specific is accounted for."

But, though a voluntary effort to distinguish between the general and the specific element in judgment may not, in general, be successful; yet it does not follow that a subject may not react more sensitively to difference in general than to specific difference, although every reaction, taking the form of an articulate judgment, should assume in consciousness a specific character. With a view of investigating this question another plan was adopted.

Three series of experiments were made: the first including differences of intensity and of pitch of sounds; the second, differences of pressure and of location of touch; the third, differences of color, of size, and of form of figures drawn on cards. The subject was made aware of the kinds of difference that would be presented to him and was asked to pass judgment, comparing the stimuli in all respects in which they could differ.

There are, then, five kinds of judgment with which an analysis of the results obtained must deal:

1. Similar stimuli judged to be the same.
2. Similar stimuli judged to be different.
3. Different stimuli judged to be the same.
4. Different stimuli judged to be different, including those in which the specific nature of the difference was wrongly judged.
5. Different stimuli judged to be different and in which the specific nature of difference was rightly judged.

If the vertical columns contain the judgments, the horizontal lines the relations of the stimuli judged, we may represent the five classes in the following way:—

Judgments.

Stimuli.	Same.	Different.	Spe. Diff. rightly judged
Same.	100–a	a	
Different.	100–b	b	c

a=percentage of similar stimuli judged to be different.
b= " " different " " " "
c= " " " " in which the specific difference was rightly judged.

If $b=a$ the difference between the stimuli is too slight to be noticed.

If $c=b$ the difference between the stimuli is so great as to be always distinguishable.

In general a series can be arranged in which b will be greater than a and less than c. This simply shows that the difference between the stimuli influences the judgment to some extent and that mistakes as to the kind of difference sometimes occur. But the fact that such mistakes occur does not of itself prove that the presence of a difference has influenced the judgment, while the specific nature of that difference has remained without effect. For the varying conditions, physiological or other, that would make similar stimuli appear different might (and generally would) also be sufficient to make one kind of a difference appear as another. We have, however, a datum that enables us to eliminate this factor. For we know the per-

centage of times that objectively similar stimuli are judged to be different in a certain specific way, and this percentage, whatever the cause of its existence, must be at least as great as that which represents the proportion of perverted judgments of difference. For example, a sound would be judged to be greater than another sound of the same objective intensity at least as often as would a sound of less intensity. The only class, then, of perverted judgments of specific difference that we can use in showing that differences may produce a correct reaction when the specific kind of difference remains without influence on the judgment, is that which remains after we have subtracted the percentage to be accounted for by mistaken judgments of similar stimuli.

From these considerations we derive the following formulæ —in which x stands for the general judgments of the nature sought; p for the correct particular judgments not explainable by chance; n for the number of possible judgments of difference presented; a, b and c having the same significance as before:

$$b = a + p + x$$

$$c = \frac{a}{n} + p + \frac{x}{n}$$

$$\text{then } x = \frac{n}{n-1}(b - c) - a$$

The experiments conducted yielded the following results:

I. Series including differences in pitch and intensity.

		Judgments.		
		Same.	Different.	Right Part. Dif.
Stimi.	Same	56.3	43.7	
	Different	26.1	73.9	46.4

Substituting in the above formula 43.7 for a, 73.9 for b, 46.4 for c, 2 for n (since difference in intensity and differences in pitch were the only judgments possible) we find x to be 11.3.

II. Series including differences in pressure and in location of touch.

		Judgments.		
		Same.	Different.	Right Part. Dif.
Stimi.	Same	47.5	62.5	
	Different	27.8	72.2	37.1

Substituting in formula as before (n again being 2) we find x to be 77.

III. Series including differences in form, color and size of figures. This series, carried out by Messrs. Hackett and Thorndike, required somewhat special apparatus. Figures of more or less complex shape were drawn on cards. The figures were either exactly alike, or differed in size, shape, color, or any combination of these. It would have been difficult to make the differences sufficiently slight to have yielded any considerable percentage of wrong judgments if the subject were allowed an indefinite time in which to formulate his judgment. The plan was preferred of making the differences fairly apparent, exposing them for a small fraction of a second only. This was done by fixing the card behind a pendulum provided with a screen; the screen being perforated by a window. The time of exposure was kept constant by keeping the pendulum-arc constant. The figures to be compared were exposed simultaneously. In 90 experiments the figures were alike; in 320 they differed in one of the above respects. The series yielded the following results:

		Judgments.		
		Same.	Different.	Right Part. Diff.
Stim.	Same	84	16	
	Different	30.6	69.4	49.1

Since there were three possible judgments of difference, n in this series is 3, whence $x = 14.5$.

It will be seen then that in each of the above series there is a certain percentage of right judgments of difference which cannot be explained as due to chance and to complete appreciation of the particular differences presented. We may refer to this as the pure general judgment of difference. This is not the place to attempt any further interpretation of this factor. That there can be an actual content corresponding to such a factor—a mere feeling of difference in general—seems introspectively clear from observations of every-day life. That such a content was alone present in the percentage of cases that we have called pure general judgments of difference we cannot pretend. All that we can say is that the subject judged as he

would have judged had this factor alone been present and had the particular determination of the judgment been an arbitrary addition of his own, not dependent upon objective conditions of stimulation. In this sense we may say that the general judgment of difference has a lower threshold than the particular.

If we ask now: What light do these facts throw upon the theory of the threshold? the following answer may be given. The main standpoint of the early psycho-physicists will seem to involve the assumption that when two stimuli were presented there was only one sense in which they differed, as also there was only one way in which this difference could be expressed by the subject. Their problem was: How great must this definite difference be in order that it may call forth this definite expression from the subject? With closer investigation of the psycho-physic methods it became apparent that a 'just noticeable' difference might mean quite a number of things, the meaning being dependent upon what was regarded as the expression of a judgment on the part of the subject. Some, like Fullerton and Cattell, would be willing to say that there was no difference so slight but that in some sense it was noticed by the subject. As the result of our own researches we feel justified in going a step further. Not only may a difference be noticeable or not, according to the way in which we define 'noticeable,' but, for any given criterion of noticeability, a difference may be noticeable or not, according to what we may mean by 'difference.' Mere difference may be noticeable at a point at which the specific kind of difference may be unnoticed. And finally, with the same criterion of noticeability, a specific kind of difference may be noticeable or not, according to purely mental preparedness of the subject to receive it. With these facts recognized, the problem of psycho-physics awaits a re-statement.

THE IDENTIFICATION OF THE SELF.

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While watching certain clinical cases my attention became attracted to the tenacity with which the sufferer would sometimes hold on to seemingly undesired, and many times actually harmful, strands of morbid experience. Why one should ever do that which, whether he be well or ill, will prove, in every way and all along, to be, not only detrimental, but distressing and dangerous, is, so far as I can ascertain, one of the problems of the Self not yet solved.

As the problem seems to be closely associated with, if not clearly involved in, that of self-identity, it appears probable that its successful study may be made in this vital connection, if anywhere.

The study of the process of the identification of the observed Self necessitates, primarily, the recognition of some fixed point, or permanent line, or 'innermost center within the circle,' of the normal self, from which departure can be noted. But to what phase or aspect of psychical experience shall we look for such a point of departure? The Self, when normal, seems capable of experiencing every sort and degree of conscious life, without necessarily disclosing the principle of its identity, or the limits within which it is circumscribed. But notwithstanding this, it seems to be commonly appreciable also, that there is actually somewhere in the personal summation a prime characteristic, which always focusses all the other, even wayward features, to a more or less stable density; and, moreover, that so long as this comes to pass, the Self actually stands out as a satisfactory realization—a persistent Identity not to be trespassed on, and likewise demanding notice and record. This focussing characteristic is discussed, usually, under Attention,

Memory, Unity, Resemblance, Synthesis, Selective Industry of the Mind, Continuity, etc.

Of the ways by which the focussing ability and its processes are accounted for in psychology, two seem to be worthy of especial consideration in this connection. One is described by saying that, upon proper stimulation, the elements of consciousness seem to 'fall together' around some one or more central characteristic, as if by their own or by its own attraction. Thus in the presence of a sensation or an idea or a motor suggestion, say of a coin, all the elements of consciousness having any predilection so to do are said to 'fall' or 'drop' into line; while, someway out of this process of precipitation, there arises the notion of a Self, identical with the one realized at previous times, in connection with other coins. As revealed by observation of the adult mind, this may possibly be the case, especially after the self-notion has become an automatic affair of many experiences. But the suspicion arises that, in reality, this is only a reminiscence of the early impulsive, and instinctive activities, becoming more vivid and more impressive as the later, more actively self-energizing processes are developed. And I take it that the actual worth of such a foundation of self-identity is very small in the ordinary adult life, so full of variableness, both within and without. Morever, the question as to whether one would ever have attained to a complete notion of self-identity, had this been the all of individual experience in this connection, is legitimate.

The other way in which the Self is said to become identified by itself is through some process of actual effort with which there is always associated a more or less distinct 'feeling of effort.' Here, instead of the elements of the Self simply falling together, at the suggestion of appropriate stimuli, they are determinedly pulled or forced together, by the Self, in conscious or possibly infra-conscious activity. The feeling of effort thus experienced is that of a real energizing in accordance with some preconceived purpose, or plan of self, or method of finding the self, in the midst of all the multitudinous affairs of consciousness; while the assurance of selfhood results from the conceived possibility of searching for and successfully finding it,

whenever it is designedly or necessarily thought of or needed. As such an assurance of self, it may be treated objectively, as a most important interest, and presented and represented in whatsoever way may seem most conducive to the furtherance of the interest in hand. Thus, if for a moment, the Self becomes lost, in no matter how complex a maze of experiential factors, and then has occasion to recover its identity, in order that its realization may be at the moment complete, it seems to be able to accomplish this very surely, instead of being obliged to wait the assurance which comes involuntarily, and always, as an heritage probably, of antecedent, purposeful experiences.

Psychology must necessarily deal with this phase of active energizing, no matter what its theory of origin, process, or of relative importance; for it enters into all *conscious* experience, and undoubtedly is the vital element in what is understood by volition, as distinguished analytically from other phases of psycho-motor activity. Latterly the tendency seems to have been to affirm the sense or active energizing or 'feeling of effort,' as being exclusively recognizable after, instead of before or during the event. It does not appear to be quite inclusive enough, however, to simply say that only as we are finishing, or immediately at the finish, of a volitional experience are we able thus to retrospectively cognize the feeling of effort. In every conation we seem to energize invariably and actively from the very moment when the focussing and fixating of attention begins to be effected; and as a matter of fact we are conscious from the very first of the processes involved. Immediately the ideo-motor need is responded to, whether the processes of response are, broadly speaking, automatic and not so clearly recognizable, or initiatory and willed, and so, more fully conscious, the feeling of effort keeps pace with the effort itself, and correspondingly feature by feature is cognized as such. Archaic as this may appear, I confess after reading and experimenting and pondering so far as I have thus been able to, my utter inability to see that any other conclusion is in accordance with the facts, at any rate as revealed in my own consciousness. Logically, how can one talk about the elements of consciousness 'falling' together, if one does not recognize the commotion

engendered? But the commotion itself has in it the element of effort—admitted by everyone; the point of difference being, as to initiation, or the place in the conative processes where it emerges into consciousness.

Conceiving, then, that the elements of consciousness may either fall into or 'accrete round' an attention-focus (automatism), or else be actually brought to such a point, by conscious effort (volition), it follows that the content of the attention-focus and of the conative processes will depend largely on the degree and kind of satisfaction, which either may be anticipated in the result or purposely required of it. Probably anticipated satisfaction is never realized from purely effortful activity alone recognized as such; always there are present certain reminiscent factors, derived from past experience, and now become incorporated in the self-feeling. But volition to be volition must always include the conscious energizing for something, which something may be added to, and so be made to contribute to, the completeness of the result so largely expected as being chiefly of a ready-made character. This something—this fresh element, which we ever seek to add to the ideo-motor activity, in order that it may be satisfactorily extended and recognized—is not a Wil-'o-th'-Wisp simply luring us on so much, as a native impulsion, a central and centering line of force, or central 'line of selective industry of the mind,' inherent in our being and constituting the assurance of a 'never-lapsing ownership,' always to be recognized as such (under ordinary conditions).

Trying now to trace and describe this central-most line-point of permanent and positive energizing, one finds it very difficult to do so in one's own normal condition, for here the psycho-physical mechanisms all run so smoothly and in such accurate balance that only as one for a time abnormalizes himself, as it were, can he catch even a glimpse of it, and then he is not very sure that what is revealed is not an illusion simply. It seems scarcely possible to observe accurately the normal self in this respect, save in the light of abnormality.

Letting oneself, then, postpone the hour for conclusions, until he shall have had an opportunity to study the problem in connection with his own relevant morbid experiences, and the

more permanently morbid states in others, clearer visions of the innermost Self and of the processes by which it characterizes itself may possibly result, even though this source may not give one the ability to formulate what one sees or to express it fully. This certainly has been my own experience, and for that matter it is obvious enough that I am not relieved of the difficulty yet. For long, as I suppose it may have been the case with many others, I caught glimpses, more or less satisfying, of the identical and identifying Self, in the very midst of its so mysterious processes of self-identification; and many times did I find myself trying to put what I had observed into appropriate language. But it was not until while conducting a series of experiments to ascertain the ordinarily warm element of different individual minds that such glimpses became assurances that there might be obtained actual evidence of a universal, more or less graspable, something, which had no business to forever escape clear apprehension and description. Granting latitude for individual type influence, people seemed to resemble one another in the way by which they bring about the attention focus; and also in the way by which they lose the ability to bring it to pass in abnormal conditions. If "the only point that is obscure is the act of appropriation itself" (James), then it began to seem to me that this very act was submitting itself to observation and was challenging some one to see and understand.

Would the studies of Professor Royce on 'Imitation' and those of Professor Baldwin on 'Mental Development' apply here; and likewise would more detailed studies of some of the patients which I happened to have on hand throw any light on the subject of the principle or process of personal identity, normal or abnormal? Whether so or not, I soon found myself under the spell of these discussions and groping about in the psychological field for cues and confirmations and helps of every kind so much needed. Applying the principle of imitation to many of the phases of misery under observation, and seeking etiological information under its guidance, and then trying to bring about curative results in the same way, I soon had to confess that never before, had things worked so easily and with such clearness of view. Conceiving that through imita-

tion of outside copies the mental processes were always being effected in definite ways, and in a centrifugal manner, so to speak, why not conceive that imitation of a self copy, derived secondarily from the primary not-self copy, should constitute, through a centripetal determination, the very act of appropriation, the self-identifying act which was the object of search? Especially did this notion seem to be required in connection with the origin, development, and course of certain diseases, and even more so still in connection with the most obstinate non-response to therapeutical measures, encountered so frequently. Here it sometimes becomes very apparent, that each section of the self-experience stream determinedly hugs to itself, and adopts and incorporates, not only whatever happens to be imitable from without, but likewise something called up, as it were, from within, and patterned after as closely as attendant circumstances admit. This, that is called up, serves as a veritable copy (or if not, why not?). The trigger pulled by suggestional impact, all the explosive energy of the Self endeavors purposely, if you please, to mimic the last, or, if not this, then some other past self-summation, in just as close feature as possible. The Process of Energizing, if we could personify it, might be said to actually call up the characteristics of the past-self, and to imitate them accordingly. And this process of energizing, this actual mimicry of Self, might be said to really constitute the process, the fact of self-identity, everywhere and always, and moreover to be recognizable as such.

The outcome of this was embodied in a paper read by abstract before the American Neurological Association and now published in full,[1] and which elucidates this general conclusion: that imitation of an extra-personal copy constitutes the initiation of many disease processes; while imitation of a self-developed and self-summated copy constitutes the process by which they so persistently and so obstinately perpetuate themselves either continuously or recurrently. This idea of the perpetuating of a self-set copy derived primarily from a non-self copy, in an imitation series, prolonged either until the original impelling force is spent or until inhibited, deviated, or overcome by the counter-

[1] *The Journal of Nervous and Mental Diseases*, New York, March, 1896.

acting influence of some more forceful copy originally derived in its own turn from without, has grown with subsequent observation, and seems to be especially applicable as thus noted to instances of hysteria, neurasthenia, hypochondria, psychalgia, imperative conceptions, and impulses, and the like, and likewise to be of much service not only in comprehending their development and nature, but in instituting and conducting methods of treatment.

But observation in this lump sort of way and under pressure of clinical necessity is not exactly of the modern experimental kind, and I suppose no psychologist gives much attention to results obtained in this way. So the need of more accurate experimentation came to be appreciated; and this prompted to various attempts to either verify, modify or annul, if need be, a conclusion so taking and seemingly so serviceable.

In order to do this I trained the attention to focus itself upon the feeling of effort when coupled with the feeling of satisfaction derived from effort of the right kind; for I conceived this to constitute the 'me-feeling,' the personal assurance, the innermost indication of normal psycho-physical activity, in myself, if not in others of a different type. At first I simply watched for that element in the passing multifarious self-experience which, common to all, might appear always to savor most thoroughly of the self-fact; and naturally I looked for evidence of unity as probably being this most surely. But soon it did not seem that *unity* of consciousness was what I would better search for after all, for in me, at any rate, there were to be noted, even during my most active moments, unquestionable gaps; in fact, that between the succeeding pulses of consciousness, so far as introspection could determine, there were unconscious blanks which must be included in any attempt to account for a continuing persistent self-identity. Of course, this does not deny conscious unity to others, or the most persistent sub-conscious activity in myself; but it does affirm that in my own consciousness blanks are to be discovered and recognized as such retrospectively, and that if each section or pulse of consciousness actually engages in 'hugging to itself and adopting the past selves' (James) it does so across certain definite hiatuses, and so does

not admit of my saying exactly that the Self as 'a closed individual' is evidenced by consciousness in this particular way. 'The inexplicable tie' is in me not of this order seemingly, so far as I have been able to discern, and continuity of self-assurance does not thus express itself as a fact.

Admitting, then, tentatively the introspective revelation that each nascent thought appropriates the past thought across an unconscious chasm, and that consciousness is at best made up of a series of waves passing and touching and being realized by crests only, and not by conscious coalescing, it becomes pertinent to investigate the manner in which the chasm is either traversed in outline or bridged over, in order that the past thought may become merged into the present, and so made to be a most serviceable part of it.

If the chasm be traversed depth by depth we may be pretty sure that at some particular depth the process ceases to be conscious; and so again we are forced to the conclusion that the only way by which past and present can be made to marry is by their being brought together over a route a part of which is always in the dark. What there is in this dark portion of the route of course does not concern scientific psychology. What was recognized on the past side of it, however, and what is recognized on the present side of it, and how these are consciously related, as they normally always seem to be, are matters of such deep concern that without solution, in some universal sort of way, self-identity itself must remain unaccounted for. From what happens, in my own experience at least, I seem justified in affirming that immediately the Self emerges from the chasm between the conscious past and the now-becoming-conscious present—emerges from momentary latency into potency—it is aware of the past as possessing certain characteristics which it would now again live over, in more interesting realization. Or if there be failure on the part of the past to be present, or on the part of the present to discern in it a definite set of features for re-living, then imagination not only rich with reminiscent elements, but with expectant ones also, furnishes them; and in either case there sets in a conscious activity toward a satisfactory realization of the self-like copy thus pre-

sented. Mostly this is done so automatically and so smoothly that no notice is or can be taken of it. The past-thought features, or the imaginative-prospect features, or the present-extra-self features so quickly and harmoniously glide into position, either more or less separately or colligated into a composite whole, that the re-living, the re-producing (re-realizing), takes place before one can be aware of it. 'Caught on the fly,' however, especially when favored by some phase of abnormality, one discovers that, from the moment of projection from the nascent point, there becomes manifest, more and more clearly, an ambitious purpose and activity to imitate the self-copy in as close feature as the varying past, present, and prospective elements admit, changing in accordance with the weight of influence exerted by each respectively. What is held up, or rather what appears during each moment or phase of consciousness as a self-copy, becomes the inspiration of a process of re-living or re-realizing it, which, if not mimicry, is close to this in fact.

A concrete illustration of this idea was timely afforded by a personal experience with 'toothache.' A hard bite upon a resisting substance gave a decided hurt at first, which was followed duly by a slight tenderness. After a number of days, exposure to bleak winds induced an irregularly recurrent pain, with comfortable spells between. Occasionally, for an hour or two, there would be no distress whatever, or a night would be passed in undisturbed sleep. Then would recur a succession of strokes, or pulses, or waves of pain varying in intensity, according to exciting circumstances, or its own essential rhythm.

What was the usual attitude of my feeling, thinking, acting Self toward this intruder? A portion of the time it was one of more or less vivid expectancy; and when this was the case much confusion as to the threshold between actual pain and not-pain was always experienced, while the self-reaction differed widely according to the intensity and degree of conscious recollection of previous twinges. Always noticeable, however, was the tendency of the neuro-muscular mechanisms to assume very similar tensions and activities with the recurrence of each attack. When the pain came unexpected, either because of a

lack of premonition or else on account of pre-occupation, it was very evident that both mind and body sought to strengthen the lines of endurance and defence, by practicing such of these reflexo-automatic devices as had been more or less usefully established in former experiences—in fact, by imitating the steps assumed under similar provocations. The involuntary fixating of chest walls, of jaws, of eyes, of attention, of emotion, of thought, and then the relaxing and subsequent seeking for relief, constituted a copy-cycle which certainly dominated largely and in detail the successive experiences, expressed most truly in terms of mimicry. Here was evidence to show that I am this moment the same Self that I was during the last, seemingly, because my innermost activity is to be during this moment similar, in a general way, to what it has been in the immediate past, or during all the series of past moments, not simply in that I have remembered the features of the past, or in that I recognize outside copies just now that were fascinating just then, or in that I imagine certain attainments possible to the future—not these alone or chiefly, but in that I can take whatever past, present or future presents, and *actively* imitate it, feature it, realize it in every possible here and now of my normal conscious existence—this it is, which gives me most assurance of my own Self, as being capable of satisfactory identification whenever needful.

Occasional needs of self-identification become sufficiently prominent to awaken notice, in connection with almost every intense experience. Marked joy, grief, anger, moodiness, depression, cerebral or muscular tension, pain, insistent ideation, active engagement, inventive exercise, reverie, etc., are instances from which to select a field for experimentation. Selecting one that promised vivid results, it occurred to me that two series of experiments, one representing the inhibitory and resisting side of life, and the other the stimulating and aggressive side, would lead on to such eventually. The toothache suggesting bodily pain for the field of inhibito-resistant experimentation, I was interested to see what would be my constant and repeated attitude toward it, both when attended to expectantly and when suffered unexpectedly. For the former I ap-

plied a drop of weak acid to the semi-abraded skin, and awaited the somewhat slow but certain development of the smart. How I felt; how I strove to blot it out mentally, by certain shrinkings, resistances, diversions; how I lapsed into simple endurance more and more, until finally fleeing to remedial relief, all together made up a striking, composite self-experience, not describable but certainly most appreciable. One experiment after three days' practice came near to being a failure because of the unexpected intrusion of a friend, whose suggestional influence was sufficient not only to largely overcome the sensational effects of the acid, but to interfere with the responsive adjustments of the self-characteristics in a natural way. And so with some of the others. However, before the ten every third-day series was concluded, I was afforded a rather convincing insight into the way in which my behavior was regularly but simply an endeavor to live over again the motions, tensions, thrills of the first and succeeding experiments. Moreover I frequently noticed a very definite dissatisfaction when, for any reason, I was prevented from doing this. As the experimentation proceeded, it became almost amusing to note the sort of strife, as it were, between the two lines of dissatisfaction—the one which came from the suffering proper, and the other arising from incomplete realization of all the self-commitance, usual to pain. But the latter always secured dominance and sort of lorded it over the former until its sway became supreme. In other words, in spite of the smart proper, the self *would have* its own total realization in the direction intended, even though the total suffering was increased thereby. The realization of the awakened Self appeared to be the very chiefest business all along. Another thing came to pass, namely: with each successive experiment the Self aimed, so to speak, to live over again not only the results of the painful experimentation, but also some of its former dire experiences not experimentally imposed. On the whole, I brought to light much that seemed to show that in the presence of pain expectantly developed, I as a self actually endeavor to reproduce all the concomitance previously associated with pain of every sort; and that my self is best satisfied when this is accomplished most accurately and fully. "If I

am to have pain," says the Self, "I want it my own way—I demand the liberty to imitate, re-live the pain-mind-body copy developed beforehand, as I please."

Arranging a mechanical device by which a smart blow on the back of the hand was given at irregular intervals and simultaneously occupying the attention as absolutely as possible, I was enabled to study the effect of pain received unexpectedly. My conclusion after the experimental series was concluded was, that repeated irregular unexpected hurts always lead in the direction of a cumulative self-copy, even if the Self does become calloused in the process; and that this more and more determines the devotion, so to speak, of the succeeding imitative Self to its own copy; and that each pulse of painful consciousness is related to the consciousness of self, through the tie which imitation itself furnishes. With attention reminiscent, expectant or held in abeyance, the disturbed self immediately flies to cover in an active mimicry of that before found most vivid, satisfactory, or otherwise determined.

Finally, my experimentation included an observed series of errors or tendencies to error while attempting to play the zither. Having but little musical knowledge or skill, any new piece of music furnished material for prolonged study. Upon first trial certain notes and strains would be played accurately and with some sort of positive satisfaction. But always there were many others upon which both fingers and ears bungled and halted most disastrously. Of course, some of the previous errors would be repeated and others corrected with each succeeding trial. In time the number would be greatly reduced and the character of each error or tendency to error would be more accurately defined. A persistent study of these latter chiefly furnished the data for the conclusions eventually arrived at, namely: that the fingering, the interest in success, the fear of failure, the general pleasure or displeasure, the expectation of pleasing others, the memory of other zither music and performers, etc., all had to do in a most egoistic way in forming a statuesque copy of variable proportions and vividness concerning which, upon each trial, all that was within me struggled to reproduce in terms of most vital and persistent mimicry; and

that, in the lasting instances of error, the error-element was the most striking characteristics of the self-copy. Repeatedly at times there was actually more satisfaction in making an error than in playing the music properly. And not until the imitable copy had become infused sufficiently with the influence of some lucky play, for instance, or of some ideational conception of success, or some anticipated applause mayhap, or some other vital and determining characteristic, did the imitation process manifest improvement.

So on many hands, seemingly, have I found tentative resort to a mimicry of a self-copy, ever renewed and ever renewable in the imitation product. And furthermore, it has appeared, more or less convincingly, that the Self is sure of itself normally, only through the bridging over from one pulse of self-sciousness to another, by the active play of imitation; and that the 'inexplicable tie,' the 'act of appropriation,' the most elusive characteristic of self, is possibly this very ability to, at any time, actually realize (*i. e.*, in the present), to a more or less recognizable degree, all that I was and as I was then (*i. e.*, in the past). And that, when the consciousness of this self-same ability is lost, no matter to what extent, nor in reference to what particular feature, always to just such an extent and in just such a way, is the possibility of self-identification gone. Normally, we can to-day imitate the self-copy of yesterday with satisfactory success. Abnormally, this ability is abridged, perverted or lost.

A side conclusion came into view; namely, that much of our so-called identity is simply fictitious, founded seemingly on our ability to imagine and affirm instead of that to prove it thus.

SHORTER CONTRIBUTIONS.

SOME MEMORY TESTS OF WHITES AND BLACKS.

The tests were made on 1,000 children in the fourth and fifth grades in the Washington Schools equally divided between the two races. The average age of the whites was eleven years, with extremes of eight and fourteen. Of the blacks, 12.57 years, with extremes of eight and eighteen years.

The tests adopted consisted of a recitation by the writer of one of four simple verses written for children by Mr. Eugene Field, followed by an explanation of all the possibly difficult words, and the recitation in concert, twice repeated, by the 20 or 40 children gathered (but never exceeding the latter number).

Each child was afterward required to repeat the verse separately in private. The degree of proficiency was marked by the system in use in the schools: E. indicating excellent, G. good, F. fair, P. poor. To obtain the average E. was valued at 100, G. at 75, F. at 50, and P. at 25.

The verses employed were the following:

No. I.

"Give me my bow said Robin Hood,
An Arrow give to me;
And where 'tis shot, mark then that spot,
For there my grave shall be."—(*Field.*)

The average of memory retention obtained by the blacks in this number was 63.22 in a possible 100 with a minimum of 48.63 and a maximum of 71.25 in the different schools, and by the whites an average of 62.54 with a minimum of 60, and a maximum of 66.

No. II.

"I once knew all the birds that came
And nested in our orchard trees;
For every flower I had a name,
My friends were woodchucks, toads and bees."—(*Field.*)

In this number the blacks obtained an average of 62.86% with a minimum of 33.75%, and a maximum of 73.43%. The whites an average of 58.92%, with a minimum of 48.02% and a maximum of 79.37%.

No. III.

"One night a tiny dew drop fell
Into the bosom of a rose;
Dear little one, I love thee well,
Be ever here thy sweet repose."—(*Field.*)

In this number the blacks obtained an average of 65.64%, with a minimum of 61.25%, and a maximum of 78.75%. The whites an average of 54.54% with a minimum of 35.81% and a maximum of 66.37%. Possibly the simpler language, the more familiar ideas, and the pleasing cadence of this number will account for the great superiority of the blacks.

No. IV.

"My Shepherd is the Lord my God,
There is no want I know;
His flock He leads in verdant meads.
Where tranquil waters flow."—(*Field.*)

(Paraphrase of the 23d Psalm.)

In this number the blacks obtained an average of but 32.93% with a minimum of 5% and a maximum of 54.40%. The whites an average of 42.14% with a minimum of 38.76% and a maximum of 45.52%. One would naturally suppose that in the verse so closely corresponding to a familiar psalm, that both races would have made a much better record than that obtained in the other numbers which were unfamiliar. It will be observed that in one black school there was nearly a complete failure (5%) and that the average obtained was 30.97% less than the combined average obtained in the other numbers, while in the whites, the percentage was but 16.52% less than the combined average. The difficulty appeared to consist chiefly in the rather abrupt changes or want of continuity in the ideas, and the use of three unfamiliar symbols. Verdant, Meads, and Tranquil, which were supported by no visual or other sense presentations, and possibly, because of the familiarity with the first line there was a failure to keep the attention alert.

The renderings given the lines were in some instances not only grotesque, but indicated an ignorance of the meaning of simple words, an entire absence of the sense of rhythm and rhyme, and a confusion of ideas. I quote a few of these renderings:

Line I.

My shepherd is the Lord my God.
"The Lord God is my Shepherd."

Line II.

There is no want I know.
"There is no one I know."
"There is no one I love."

Where the only one I know.
"There is no other God before me."
"I know there is one."
"There is no one like me."

Line III. His flock He leads in verdant meads.
"He leads His flock in vast fields."
"He leads his flock in needant fields."
"He feeds His flock by night."

Line IV. Where tranquil waters flow.
"Where vast waters flow."
"Where trinkling waters flow."
"Where trennikel waters flow."
"Where peaceless waters flow."
"Where tremble waters flow."

As may be inferred, the false renderings were more frequent in the blacks because of their more limited vocabulary and feebler comprehension of the language.

In confirmation of the theory that the capacity for intellectual achievement depends very largely upon the acquired control of the attention and the degree of memory susceptibility or concentration, I find striking evidence in the close correspondence of the average markings for study rank, made by the different teachers and the markings for memory rank made by the writer.

The average obtained by the blacks in studies, being 64.73 in a possible 100, and in memory 58.27, and by the whites, 74.32 in studies, and 58.09 in memory. In making comparisons allowance must be made on the one hand for excessive and insufficient marking on the part of teachers, and on the other for the fear and embarassment caused by reciting to a stranger.

I am, however, convinced that if the study and memory markings were made by one person that they would correspond still more closely; and that it is possible to determine by the memory test the capacity of the teacher to instruct, as well as that of the pupil to acquire; as memory depends upon the habit of attention, to the growth of which the intelligence is proportionate (Ferrier). Another feature in the results obtained is the very remarkable and unexpected correspondence in the memory rank of each race, 58.09 in the whites, and 58.27 in the blacks; from which, in the absence of any data for comparison, nothing can be positively assumed.

To summarize the results, the average age of the whites was 11 years, of the blacks, 12.57 years, a difference in favor of the blacks of 1.57 years. The average obtained by the blacks in study rank was

64.73, in memory rank, 58.27, a difference of 6.46%. The average obtained by the whites in study rank was 74.32, in memory rank 58.09, a difference of 16.23%. The average obtained by both races in study rank was 69.52, in memory rank, 58.18, a difference of 11.34%; in study rank the memory rank of both races being equal.

If Ribot is correct in the opinion that voluntary or controlled attention in distinction from "spontaneous or instinctive attention, is a product of civilization, an instrument that has been perfected," we have, in these races as found in Washington, an apparent equality in development.—(The Psychology of Attention.)

No positive conclusions can be drawn from these tests as to the increase or decrease of mnemonic capacity in the whites, as to my knowledge, there is no standard of comparison of the same number and ages. In both races, of course, the memory is in decadence from primitive conditions, but as the blacks are much nearer those conditions I naturally expected to find a much greater auditory mnemonic ability than is possessed by the whites.

The decadence in the blacks is serious enough to be easily recognized by those familiar with the race in slavery, or in the early days of freedom, or with the isolated communities of illiterate negroes of purer blood still to be found in portions of the South.

The enfeeblement of the memory is accompanied in both races by a parallel decline in the powers of sight and hearing, and is apparently due to neglect in training the attention and of compulsory exercise of the memory, to educational methods which foster an increasing dependence on technical aids to mental effort, to the abnormal increase in visual presentations to the memory, until they exceed and displace all other sense presentations, to the neglect of mnemonic training, and to the mental paralysis consequent upon a too complex and overloaded curriculum.

The children selected from the blacks for this test were the darkest to be found in the schools. Of these, 72% were classed as 'dark,' 22.80 % as 'medium,' and 5.20% as 'light'; but such a classification is extremely arbitrary, as the degree of admixture of blood cannot be determined by the complexion, and negroes of absolutely unmixed blood are rare in our large centres of population.

It would be interesting to have examinations made in smaller communities of blacks of much purer blood, in isolated districts like the sea islands on the Atlantic seaboard. The memory rank of those subjected to this test is, undoubtedly, lessened by the admixture of blood, which tends to equalize the conditions; but, as will be ob-

served, their memory rank in its decadence much more frequently exceeded the study rank than among the whites—an excess also due to their acknowledged racial deficiency in reasoning power.

The intellectual deficiency of the blacks as compared with the whites is more particularly shown by the average ages at which the grades were attained by both races; the blacks having attained the fourth grade at 12 years, and the fifth grade at 13.14 years, while the whites attained the fourth grade at 10.63 years, and the fifth at 11.40 years, a difference in favor of the whites of 1.37 years in the fourth, and of 1.74 years in the fifth grade, differences which indicate the value of heredity in racial culture even if ability to memorize be equal.

It is not within the purpose of this paper to deal with educational methods, but rather with results.

In studying the latter, it is painfully apparent "that the fundamental, discouraging and almost insurmountable difficulty in the education of the Negro is his ignorance of our language; in his home and in the field, in the church and school, he speaks a *patois* without a literature, and with a very limited vocabulary. The great mass of the Negro population of the country very rarely hears the English language spoken in its purity, and the children fortunate enough to be taught by one of their race who has acquired it, only hear it in the school room or in the houses of their white masters." (Article, 'The Negro and the Church,' by the writer, *Prot. Epis. Rev.*, July, 1896.) In both races there is a better knowledge of the signs or symbols than of the things signified, or an imperfect mastery of the language. Among the blacks, it will especially be found that many words in very simple prose and verse convey no conception of the thought or object represented. They also exhibit a decadence of the observing faculties from earlier conditions.

In both races there is a too great reliance on exterior aids, and a neglect, deficiency, or failure in habits of thought or the ability to think clearly, which makes it difficult for a child to use its own natural mental gifts, and which ultimately results in a loss of brain power and an inability to assimilate, or to determine the general principles to be derived from the great variety of particular knowledge presented in our school curricula.

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EXPERIMENTS ON MEMORY TYPES.

It has been said that "the great mystery of memory lies in the mind's apparent power to transcend time and bring itself into contact

with the vanished past." It is not the purpose of the present paper to solve the 'mystery of memory,' but to show how memory, under certain conditions, acts. This article will give the results of experiments tried upon auditory and visual memory.

Auditory-Memory. The following experiment was tried on public school children, students in a commercial school, and juniors and seniors in a college. Three groups of numbers were chosen, each group consisting of ten numbers. The first group was read at the rate of one number per second. The second group was read twice, and the third group three times, each number being pronounced at the same rate as those of the first group.

The purpose of the experiment was to find how much the memory would be strengthened by the second and third reading. The experiment was tried several times upon a class of seventh grade pupils and the results were as follows: They numbered 52% of the total number with one reading; 50% with two readings, and 58% with three readings. Sixth grade pupils gave the following: 42% of the total number with one reading, 41% with two, and 54% with three readings. The experiment tried upon the commercial students varied a little from the sixth and seventh grade pupils, giving these results: one reading 36% of total number; two readings 37%, and 54% with three readings. The experiment which was tried upon a class in psychology at San José College, gave the following results: One reading 58% of total number; 44% of total number with two readings, and 66% of entire group with three readings. Some results obtained from a class in psychology at Napa College were as follows: 48% of total number with one reading, 40% with two readings, and 65% with three readings; or, to put them in tabular form, they would be as follows:

Class.	I.	II.	III.
7th Grade.	.52	.50	.58
6th "	.42	.41	.54
Com. Stud't.	.36	.37	.54
S. J. C.	.58	.44	.66
N. C.	.48	.40	.65

These figures seem to indicate that two readings nearly always—there being but one exception—weakens the memory. I noticed in trying the experiment that after the group was pronounced once and was being pronounced again there nearly always appeared a look of confusion in the faces of the pupils. In the first reading associations

would be formed, associating certain numbers together and giving them a certain order, such as might be suggested as they were being read. But, when they were read the second time, new associations would be formed, and some of the former ones lost, thus making a confusion in their thought and causing them to lose all the associations they had made.

In every case there was a decided improvement with three readings. This may be because the third reading harmonized the associations made in the two previous readings and thus the confusion that was caused was dispelled.

The data obtained shows that the power of retaining or recalling the memory-image varies with different persons. Some were able to recall the entire list while others could only recall two or three numbers. But the fact that one person is better than another in reproducing memory-images, it does not indicate that he has a better mind, as is thought by some people. Usually the boy or girl with a 'parrot-like memory' pleases everybody, while he who has to cognate and con over what he wishes to remember does not stand very high in the popular estimation.

It was found that younger pupils nearly always reproduce the numbers without any hesitation, while college students always hesitated and required more time to reproduce the list. This would seem to indicate that pure spontaneous memory depends on the degree of impressibility, while a cultured intellect depends more upon the power of reason. Young children are superior to men in impressibility, in the power of retaining the memory-image, and in spontaneous recollection, while the latter have gained more power of voluntary acquisition and recollection. Hence the training of memory should not only be to increase the power to recall an image, but the power to determine *what* shall be recalled.

Some experiments seem to indicate that the power of recognition is nearly double that of recall, that is, if some of the numbers not reproduced be mentioned, they are recognized in nearly every instance.

Visual-Memory. Two lists of words, consisting of fifteen each, were chosen, care being taken in the arrangement of the words.

They were as follows:

I.	II.
Bottle	Table
Pen	Man

I.	II.
Coat	Book
Floor	Room
Gun	Cat
Picture	Desk
Stove	Spade
Ring	Ship
Tree	Knife
House	Carpet
Rock	Lamp
Sun	Pencil
Bridge	Window
Hill	Fan
Chair	Mouse.

The two lists were placed upon the blackboard and covered. The cover was taken from the first list and it was exposed for thirty seconds. The second list was exposed at the rate of one word every two seconds, the word being erased at the end of that time.

The object of the experiment was to determine which was stronger, successive or simultaneous memory.

It was tried upon pupils of a public school, ranging from eight to fifteen years of age, and the following results were obtained:

Age.	Sim.	Suc.
8	.11	.33
9	.32	.49
10	.33	.49
11	.44	.52
12	.55	.56
13	.53	.54
14	.56	.56
15	.51	.52

The table seems to indicate that successive visual-memory is much better for the younger pupils than simultaneous visual-memory; but as they increase in age they improve in the power of the latter, there being a difference of 22% at the age of eight, while there is a difference of only one per cent at the age of fifteen.

It will be noticed that the increase in the power of recall is not uniform throughout, those thirteen years of age being below those of twelve, and those fifteen less than those of fourteen. This is owing

to some idiosyncrasies of the individual pupils, but does not effect the general result.

The reason the younger pupils recall more of the second group is, probably, because their power of association is not so great. Their power of reproducing the words depends upon their powers of retention and the degree of impressibility, while the older pupils depend more upon association for retaining the words and their volitional powers to recall them. In the second group the opportunity offered for association is not so great as in the first, and hence, more are remembered by the younger pupils.

A similar experiment was tried on a class of commercial students, but numbers were used instead of words. They varied from fifteen to twenty years in age, and it was found that simultaneous was much stronger for them than successive visual-memory, there being 42% of the entire list reproduced when the fifteen numbers were exposed thirty seconds, and 39% when each number was exposed two seconds.

This seems to indicate that older students and those more advanced can recall more objects when exposed simultaneously than when shown successively. It is owing to the fact that the power of association is stronger in the older than in younger persons.

Visual- and Auditory-Memory. This experiment was tried on public school children. Two groups of names were used, each group consisting of ten words. The first group was read at the rate of one word every two seconds. The other group was placed on the blackboard and covered. Each word was then uncovered at the rate of one every two seconds and erased at the end of that time.

The object of the experiment was to determine which was the stronger, visual or auditory-memory.

The data obtained seems to indicate that generally auditory-memory is much stronger for younger pupils than visual-memory. The following results were obtained:

Age.	*Auditory.*	*Visual.*
8	.42	.30
9	.54	.57
10	.57	.54
11	.69	.66
12	.80	.65

There is but one instance in the above table where the visual-memory excels the auditory, and this was probably due to a lack of attention by a few pupils.

The younger children are accustomed to a large amount of auditory work, and it is natural that they should remember spoken better than written words.

It is probably true that the auditory-memory is associated with the visual. I noticed, in trying the experiment upon some pupils, that they pronounced the words softly as they were exposed. Thus, if "when the two senses act together in recollection they hinder each other," we must observe this in accounting for the fact that visual is weaker than auditory-memory.

A similar experiment was tried upon a class of commercial students, ranging from fifteen to twenty years in age, gives 28 % of the total number reproduced in auditory and 45 % in visual-memory. This would seem to indicate that visual-memory is better for older students than auditory-memory. The reason is because visual-memory is employed more by them than auditory.

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THE PROPAGATION OF MEMORIES.

One essential condition of the continuity of individual consciousness in our present state seems to be the persistence of impressions in the substrate, or, in other words, the retention of vestiges. It is apparently usually implied, if not stated, that this persistence is due to the survival of the cells (of the cortex presumably) in which the original impression issuing in consciousness was made. Were this theory correct the destruction of a given cell or generation of cells would banish forever their vestiges and destroy the power of reproduction in so far. It is, however, a notorious fact that the events of a very early period of life are remembered and vividly conceived of even in very late life, and it is not unusual for the earlier events or experiences to crowd out the later ones which ought, by reason of their freshness, to be most prominent. Any theory of memory which depends on the persistence of the original elements is weak, in view of the theoretical consideration mentioned, and is contradicted by facts of observation. In the first place, the law of substitution and restoration of function is well intrenched in the data of pathology and experiment, and, in the second, we have every reason to believe that the individual cell of the cortex has its birth, adolescence and period of decline, like any other cell, and it follows that its vestiges, as such, disappear with it.

Recent investigations show that a comparison of a given number of ordinary brains with those of insane persons reveals little or no difference (if cases of general paralysis and alcoholism be excluded). In other words, there are nearly as many cells showing pigmentary degeneration and vacuolation in one series as in the other. Any section of the cortex will contain some cells evidently beyond their prime. With this phase of the subject is associated another; viz, the power of multiplication. Some years since the writer described a process of proliferation by which the cerebellum in particular is supplied with a germinative zone which, by constant subdivision of its elements, gives rise to the definitive cells. The announcement met with incredulity on the part of critics, but has recently been fully corroborated by several independent investigators. At the same time the writer suggested that, in many groups, germinative or proliferating areas or nuclei persist, and from these cells arise from time to time to supply later needs. The details of this mechanism remain to be studied, but Professor Howard Ayers recently corroborated the existence of subdividing cells in the mature brain, and similar cases are reported by others. So far from exciting surprise, this may be expected. Many histologists have expressed surprise at the comparatively new doctrine of nervous transmission by contiguity rather than continuity, but it is not difficult to see that this method of non-exclusive reaction of cell upon cell is exactly adapted to permit the graceful withdrawal of a cell as its age approaches, while a younger element gradually ingratiates itself into the sphere of influence of the other by the growth of its fibrous arborizations. True, there doubtless are latent cells in the cortex, and it is not necessary to suppose that rapid proliferation is continued throughout life. All that is insisted on is that the mental life of the person is not coextensive with that of the cells in the cortex.

If it be granted that the individual memory outlives any single generation of cortical cells it is evident that a theory of consciousness is called for other than that commonly in vogue. I have elsewhere outlined such a theory. Briefly, it is presumed that the unity of consciousness is not due to the flickering of some extraneous illumination upon part after part of the cortical reservoir of vestiges, but that the individual consciousness at any time is the totality of the interrelated activities or the associational equilibrium. The histological mechanism of the cortex leaves no opportunity for doubt that an excitation of one region does habitually produce the most extensive set of interreactions with other regions, and we may best conceive of the outcome as

a neurological equilibrium, always varying, but always a single, though complex, correlation. The metaphysical concomitants of this view may be passed over here, though of vast importance to monistic thinking.

If, then, it be admitted for the argument's sake that consciousness is a unit of equilibration on its neurological side, it may be farther understood how the early vestiges may be propagated beyond the life of the first generation of cells, *i. e.*, those which first received a given vestigeal impression. When, *e. g.*, a certain event is recalled, a succession of equilibriated states occur to which the several cells concerned contribute each its share. When a new cell is gradually interpolated in the cortical associational system it participates in this kind of an equilibrium, at first passively and afterwards by reproduction of the secondary vestige due to its early functioning. It being of the same order as the cell which is now aging, it reacts after its kind, *i. e.*, in a manner suitable to its position as to the other cells and avenues of discharge. Thus, in time, we conceive that a nearly complete substitution for an old cell may be affected without our perceiving any difference in the tone of memories. That there is such change is unquestionable. What the man recalls of his boyhood is something very different from the memory of the same events soon after their occurrence.

It is no longer difficult to understand why in old age the events of youth may re-emerge in memory. The earlier events have their relations with the simpler, broader forms of association, and when the subtle, later connections fall away, older forms of equilibrium reappear. To be more explicit, in the first few years of the child's life the avenues of association are relatively few and they are increasingly complicated with every new element introduced into experience. The tendency of all early experiences is then to follow lines already open. The little events of childhood are superposed upon the elemental associations. Now, when the more complicated associational paths are destroyed, suggestions tending to awaken vestiges are shut up to a few lines of association, and these are just the ones on which the childish experiences were based. It is not necessary to pursue this line of reasoning further. It is believed that most of the hitherto unreconcilable facts of memory are consistent with this theory of propagation of vestiges.

C. L. Herrick.

Denison University.

NOTE ON 'REACTION TYPES.'

The following observations are of so fragmentary a sort as to seem hardly worthy of publication, yet the results are so striking and suggestive and it being impossible to complete a more careful series, it has seemed best to present them and let them stand for what they are worth.

During a visit to the laboratory in November, 1896, of the well-known pianists, Rosenthal and Sieveking, who were at that time giving concerts in New York, their reaction-times to sound were measured with a Hipp chronoscope. The signal was the tap of a metal hammer, and they were told to raise the first two fingers of the right hand as quickly as possible, no instructions as to direction of attention, etc., being given. It may be said that neither of the gentlemen had ever seen a reaction-time apparatus previously, and the tests were made on each without the knowledge of the other, thus precluding the possibly disturbing intervention of professional jealousy. A series of seven records on Rosenthal resulted as follows (times in σ); 110, 118, 119, 112, 119, 123, 123, giving an average reaction-time of 117.7, and an average variation of only 3.8. When questioned as to the direction of his attention during the tests he stated emphatically that it was entirely directed toward the signal and doubted, whether he could hold his attention upon the reacting fingers. When requested to do so a series of four reactions resulted as follows: 250, 230, 270, 268, an average of 254.5, and average variation of 14.5 *i. e.*, an average reaction-time of more than twice that of the first series, and an average variation of about four times that of the first. A second series of seven reactions with the attention upon the stimulus resulted as follows: 105, 107, 106, 101, 106, 110, 114, an average of 108.4, and an average variation of 3.9.

But one series of five observations was made with Sieveking and, as in the former case, without instructions as to attention. The results were 114, 114, 117, 120, 118, an average of 116.6 and an average variation of 2. When questioned as to attention, Sieveking was as emphatic as his rival but declared that his attention was entirely concentrated on the reacting hand, indicating the interossei of the first and second fingers as the muscles used; which statement while anatomically incorrect was psychologically entirely satisfactory. When asked to react with the attention turned to the signal, he attempted to do so but declared it impossible and declined to proceed.

It is greatly to be regretted that the observations could not have

been pushed further and carried out systematically, but circumstances put it out of the question and it must be said also by way of apology that the experiments were made more to exhibit the apparatus than to obtain results and without the idea of publication. Nothing could have been more striking, however, than the clearness with which both gentlemen grasped the point at issue and described their states of mind. It would be interesting with longer series to interpret them in the light of the peculiar musical training and characteristics of the two artists which are said to be diametrically opposed.

As it is, one can only say that, so far as single observations are of value, their reactions argue vigorously for the affirmative in the debated question of the existence of types of reaction.

LIVINGSTON FARRAND.

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As Professor Wundt devotes many pages of the fourth edition of the *Physiologische Psychologie* to sensory and motor reactions, I hope that he will find space in the fifth edition to include cases such as these. He writes in the fourth edition "Cattell (*Phil. Stud.*, viii, S. 403) * * * konnte weder bei sich, noch bei einigen andern Personen einen von der Richtung der Aufmerksamkeit abhängigen Unterschied finden." But of the three cases reported by me in the article published in Professor Wundt's *Archiv*, one had a motor reaction nearly half again as long as the sensory form with corresponding differences in the mean variations. Professor Wundt indicates that I was not competent to react in a sensory fashion. He writes further "Dies schliesse ich aus der Vergleichung meiner eigenen Versuche, in denen ich sensoriell reagirte, mit denen Cattell's, der sich offenbar der muskulären Reactionsweise bediente."[1] Now Professor Wundt can evidently make an '*experimentum crucis*' for his own case by going to his laboratory, directing attention to the movement and reacting in half the time and with half the irregularity of his early experiments.

Of Dr. Farrand's cases Rosenthal is a performer of extraordinary technical expertness. His fingers are so perfectly trained that he does not need to give them the least attention. Sieveking, on the other hand, is impulsive and emotional. I do not think that we should be justified in concluding from these experiments that Rosenthal is an 'audile' and Sieveking a 'motile.' It seems to me rather that my or-

[1] As a matter of fact my reactions are not 'motor.' They are 'sensory' when the stimulus is very weak or very strong, but in general the attention is diffused, but little being directed to the experiments.

iginal suggestion is supported—namely, that people react most quickly and regularly in the way in which they are used to reacting, and that when the reflex character of the reaction is disturbed the times become longer and more irregular.

J. McKeen Cattell.

It may be added that Professor Wundt's position remains unchanged in his later published *Outlines* (pp. 198 ff.), while Professor Warren has indicated (Psychological Review, Nov., 1896) the same attitude in Professor Titchener's excellent *Outline*. It seems worth while to say this since cases *galore* have now been reported (by Flournoy, Angell and Moore, and myself, besides those reported above, and under more exact experimental conditions); and they seem fully to meet the call for 'facts' made by the last-named writer, at least, several times over (see his *Mind* articles). The existence of 'types' of simple reaction can no longer be ignored by any one.

J. Mark Baldwin.

PSYCHOLOGICAL LITERATURE.

Lehrbuch der Psychologie. FRIEDRICH JODL. Stuttgart, Cotta'sche Buchhandlung, 1896. Pp. xxiv+767.

Here is a new systematic work on psychology, 750 pages strong. It is by Professor Jodl, now of the University of Vienna, but for many years of the University of Prague, well known in this country for his ethical writings and as one of the editors of the International Journal of Ethics. The reader observes, first, and not without a quickly smothered feeling of gratitude, that the book has not a single illustration, not a curve nor a diagram, not even a chart of the brain, nor a cut of Zöllner's figure. The second striking feature is the wealth of references to psychological literature. After every subject and after every subdivision of a subject, there follows a paragraph citing the special literature upon that subject. At the end of the book there is a collected bibliography of all the works cited, comprising almost nine hundred titles. Not only by his references, but also by his discussions, the author shows himself to have an accurate acquaintance not merely with German psychological literature, but also with that of France, England and America. This is especially true of English and American works, by which the author has evidently been much influenced, and he delights in comparing German and English terminology.

The work is divided into two parts. The first, introductory, treats in three chapters of the scope and method of psychology, of the relation of body and soul, and of the classification and description of conscious phenomena in general. The second part is divided into nine chapters, as follows: IV. Sensations: their forms, laws and methods of measurement. V. Special sensations: organic, kinæsthetic, cutaneous, smell, hearing and sight. VI. Feelings of the primary order: sensuous feelings and elementary æsthetic feelings. VII. Elementary phenomena of will. VIII. Secondary phenomena: memory, association and representative attention. IX. Most important products of the imagination: time, space, subject and object. X. Language and thought: origin and spirit of language, word and idea, judgment and reasoning. XI. Feelings of the second and third order:

feelings of personality, æsthetic and moral feelings, etc. XII. Phenomena of will of the second and third order.

In reading or reviewing Professor Jodl's book it is necessary to remember that it is a *text-book*, written for purposes of instruction. If, therefore, it should be found to contain little that is striking or original, this would be its greatest merit. It is often asked whether psychology is yet a science. Nothing could be so discouraging to those who have cherished a hope that it is, as the *individual* character of the various outlines, elements and text-books that have appeared during the last ten or fifteen years. So many books, so many sciences of psychology. Some are mainly physiological, some scorn to mention physiology. Some attempt no general classification of mental phenomena, all the others give different classifications. Some are mere picture or story books apparently designed to astonish the reader. Many of these works are extremely valuable contributions to psychology, presenting new experimental methods and results, new schemes of classification, and new criticisms of former errors, but they are contributions merely and not text-books. They are indicative of the formative stage of a science. The present work by Professor Jodl is less characterized by new and striking features. The material brought forward is, with some exceptions, that which is common to all schools. If this is not indicated in the following review, it is, of course, because it would be apart from the purpose of a review to dwell on tiresome details.

In the first chapter, after defining psychology as the science of the forms and laws of the phenomena of normal consciousness, which appear in the adjustment of living organisms to their environment, the author describes the various methods by which psychology may be studied. Then follows an interesting chapter of 50 pages on soul and body. It contains the briefest statement of the structure and form of the nervous system, a single paragraph on localization, and a long discussion on the nature of consciousness and its relation to the body. The standpoint is thoroughly monistic. Mind is not a substance, nor anything that can outlast the body or even outlast peculiar cerebral conditions of which states of consciousness are inner aspects. Mind is a convenient term for the sum total of these states. Conscious states are not, however, to be identified or confused with physical states or physical beings. The parallelism between them is not that of two different series, but of two aspects of the same series. There is no dualism of body and mind; the dualism is one of perception. But the author does not admit an inner aspect to all physical being

and calls panpsychism a new mythology. Consciousness belongs only to organized living bodies, accompanying certain conditions of cerebral development. Without organic life, no consciousness. But the reverse does not follow. No causal relation exists between mind and body. Causal relations exist only between neurological processes on the one hand and conscious processes on the other. But, indeed, we must not think of it as if there were two series. There is one only. In our description, we may follow either the outer objective aspect, *i. e.*, the brain changes, or the inner, subjective aspect, or conscious states, as now one, now the other is more permeable. At present, we are largely confined to the latter method, owing to the extremely backward condition of cerebral physiology. The endless difficulty about unconscious mental states disappears in a minute when we consider consciousness as an attendant phenomenon upon neurological processes, which is present under definite conditions only, but which always presupposes nervous activity. All unconscious mental action may be relegated to physiology. On the whole, concerning the author's presentation of the relation of mind and body, a 'materialist' would have to be of the so-called 'crass' order to find much in it that would be highly objectionable.

In the third chapter follows a more critical analysis of the meaning of consciousness and its forms. The opposition of subject and object is the fundamental postulate of all consciousness. Presentation (Wahrnehmung) is the very essence of consciousness. In general what we mean by consciousness is a succession of such presentations with the various relations between them. In this connection, the author discusses the question whether the ontogenetic development of consciousness is an abridged recapitulation of the phylogenetic, and concludes that the evidence for such parallelism is yet insufficient. Sleep, somnambulism, and hypnotism are briefly discussed in this connection, but with a curious neglect of their cerebral accompaniments, which ill accords with the author's theoretical standpoint.

Under the title, elementary forms of consciousness, the author discusses the problem of classification. After criticizing the attempts of Horwicz and Spencer, Schopenhauer and Wundt, Münsterberg and Lehmann to find the primary elements of consciousness in feeling, will and idea respectively, he decides upon epistemological grounds for the three-fold division into sensation, feeling and will. (Empfindung, Gefühl, Strebung.) The basis of this division is found in the antithesis of subject and object which is the very condition of consciousness. It gives us action (sensation), reaction (will), and the

connecting central excitation with its accompanying pain or pleasure (feeling). This classification, thinks our author, conforms to the functioning of the nervous system itself. In sensation we have the mental accompaniment of a stimulus from without by way of the sensory nerves. In feeling we have the *value* of the stimulus for the good or ill of the organism. In will we have the reaction expressing itself in movement through the motor mechanism.

Sensation, feeling and will being thus designated as primary phenomena, we may denote all reproductions of these as *secondary* phenomena. Under this head is included the memory-image of every kind, and for this memory-image or mental state of the second order, we may reserve the term Vorstellung. Finally the author distinguishes phenomena of the *third* order, namely those highest mental processes resulting from the fusion and elaboration of phenomena of the first and second order, such, for instance, as conception, thought, and the constructive imagination.

The first part closes with a description of 'the objective spirit'—a phrase which, rescued from its empty Hegelian meaning, the author uses in the later scientific sense of Lewes, Lilienfeld, and Carus. It signifies our mental environment, the spirit of the age as expressed in human thought, art, science, etc. It is this objective spirit, together with the organic inter-relation of successive generations which presents the element of truth in what has appeared in ancient and modern mythology as the idea of personal immortality, an error which has arisen from mistaking a mere abstraction, the soul, for a real spiritual being or substance.

In the second part, two hundred pages are first devoted to sensation. The distinction which modern English psychology makes between sensation and perception, is recognized under the form of the receptive and spontaneous consciousness. In the relating, comparing, and discriminating spontaneity of consciousness we have not to recognize any new or mysterious 'powers' of mind. That which is originally given in consciousness is not a series of isolated sensations which may be related and compared, but a complex of related sensations which may be analyzed. The doctrine of the specific energy of the nerves in its older form is severely criticized and the oft-repeated teaching that electrical or mechanical stimulation of the end-organs of taste, smell, or even of sight or hearing, is directly followed by their specific sensations, is declared to be doubtful. Synæsthesia is found to be an incorrectly named phenomenon and to have a purely psychical explanation. A section on the psychophysic law follows. This is a

mere discussion of the validity of the law and its meaning. There is no detailed description of experimental methods. The results of late experiments in the value of psychophysical constants for the several senses are not given. The author rejects the psychological interpretation of Weber's Law given by Wundt and does not decide between the other two.

Then follow 140 pages devoted to special sensations, namely, organic and cutaneous sensations, and sensations of movement, taste, smell, hearing and sight. No mention is made of the semi-circular canals as organs for sensations of equilibrium. The reader is referred to other works for all details of experiments and experimental methods. The section on visual sensations is particularly exhaustive. The author, following Hering, Stumpf and James, strenuously maintains that a spacial quality, even tridimensional inheres in visual sensations in their native purity. The empiricists are not altogether wrong in emphasizing the educating influences of other factors in completing our conception of spacial relations, but they are wrong when they give to inference or judgment, which are secondary mental processes, the mystic power of creating something, *i. e.*, the space idea, out of nothing. Indeed, he distinguishes in sensations of sight three distinct elements, intensity, *i. e.*, brightness or darkness, quality, *i. e.*, color, and extension. He admits pure quantitative changes in color sensations without change of quality.

After sensations, the author treats of feelings of the primary order, *i. e.*, sense feelings. Pleasures and pain are their essential qualitative marks and each of these can only be graded quantitatively. They are both positive. Sense feelings, cutaneous pain, for instance, are not to be confused or compared with sensations. They are totally different forms of consciousness, accompanying and interpreting the sensations, and appearing later. The author does not consider or even mention apparently serious objections to this theory, such as have been educed by von Frey, Griffing and Nichols.

In the section on the elementary æsthetic feelings, it is shown that, apart from all representative or associational processes, pure æsthetic pleasure is derived from colors, tones, and from harmony, rhythm and proportion. In the last analysis, the pleasure may be traced to that condition of nervous activity lying between excessive and deficient stimulus. This is very good, but the law is merely mentioned in this general form and no attempt is made to carry the analysis further after the manner of Helmholtz or Grant Allen. This is an example of a certain tantalizing tendency, shown throughout the whole book, to stop just short of the final analysis.

This failure to grapple seriously with psychological problems is still better shown in the next chapter on the primary phenomena of will. According to the author's division of mental processes, the primary phenomena of will are to be distinguished from *feelings* and from *sensations*, including of course all muscle sensations. As they are *primary* states, they are also to be distinguished from all representative elements, *e. g.*, motor images. After these exclusions, one wonders just what there will be left to constitute the primary phenomena of will. Of course, they must be purely psychical conscious phenomena, and when the author refers to them as *movements*, as he often does (*e. g.* in III., 64), we must suppose that he is referring merely to the objective correlates of the purely mental processes. Pure and simple *effort* (Streben) is the final result of this analysis, and this is an ultimate primary phenomenon of will. But when the author attempts to describe this, it becomes as difficult to separate it from phenomena of feeling and sensation, as it is indeed difficult to do so in our own inner experience. In fact, he naively admits that effort is a general term for those mental states characterized by the feeling of bodily needs and the reactions consequent upon them. We cannot but think that the author's attempt to find in will any primary psychical phenomenon is a failure. His loose and careless treatment of the will is illustrative of the treatment of this subject in nearly all the psychology books of the day, and in striking contrast to the incisive and analytic treatment of it in Külpe's *Grundriss*.

Exposition of the secondary phenomena is begun in Chapter VIII. Reproduction, Memory and Association are clearly and fully treated with satisfactory recognition of the physiological principles involved.

On the whole this is the best systematic work on psychology that has recently appeared in Germany. Its purpose does not admit of comparison with Wundt and Külpe. It may be compared with Höffding whose work it will supersede. It is, to be sure, a compilation, but a most valuable one. The author is evidently neither a physiologist nor an experimentalist, but he is in sympathy with experimental methods, and he handles his subject with a fairness and a wideness of vision, with which a life spent upon the details of laboratory work would hardly be consistent. One notices also a grateful freedom from long and tiresome discussions of disputed questions.

G. T. W. PATRICK.

UNIVERSITY OF IOWA.

Elements of Psychology. GEORGE CROOM ROBERTSON. Edited from notes of lectures delivered at University College 1870-1892 by C. A. FOLEY RHYS DAVIDS. New York, Charles Scribner's Sons, 1896. Pp. xvi+268. $1.00 net.

Robertson was a man whose influence was greater than his reputation. Natural ability of the highest order, trained by thorough study, wide reading and careful thought, was subordinated to a character conscientious and generous to a degree not only rare, but in my own experience unapproached. On a sick bed recovering slowly from painful surgical operations or traveling in the vain effort to regain health, he was always able to give more than he asked. Carrying for twelve years the burden of a disease that must prove fatal, devoting his best energy to teaching his classes at University College, to helping his friends and forwarding scientific and social movements, editing each number of *Mind* as though it were a newly discovered MS. by Aristotle, it is no wonder that the amount of his published work was small. Neither is it surprising that the friends and students of such a man should wish to preserve all that he left, even the oral instruction to his classes, existing only in the notes of students.

These lectures on psychology and the second series on philosophy would have been viewed by Robertson himself with mingled feelings. He would have appreciated the piety which lead to their compilation, but, careful and conscientious to an extreme in all that he wrote, he would scarcely have sanctioned the publication of extemporary remarks preserved by the notes of students. A book such as this can scarcely be judged by ordinary standards. As a memorial volume it will be dear to Robertson's friends; it shows that his teaching was stimulating to an unusual degree. The contributions to psychology as a science are not great. Robertson follows his teacher Professor Bain; even his use of the German psychology and his own ideas are often brought forward as criticisms of Bain's writings. Probably Robertson would have regarded as his most important contribution the elaboration of a theory of perception through the muscular sense, but this is not likely to maintain a permanent place in psychology. There are, however, many apt thoughts and suggestions, which with the general point of view—that of traditional English psychology brought in touch with the latest continental work—make the book one that will be read with profit by all teachers of psychology.

The notes are, however, called 'Elements of Psychology,' and the book is placed in a series of text-books. At first sight it seems to be unsuited to this purpose. The lectures are based on and presuppose

other text-books—Bain, Sully, Murray, or Höffding. The style is colloquial as reported verbatim by students and pieced together by the editor, with occasional interpolations in a very different style from Robertson's manuscript notes. There are repetitions and awkward phrases and even mistakes. Yet in spite of all this, when the work is compared with our most recent text-books, as Wundt's *Outlines* or Titchener's *Elements* it shows great freshness and originality. Perhaps there is no better introduction to psychology.

J. McKeen Cattell.

The Philosophy of Theism. Alexander Campbell Fraser. Gifford Lectures Before the University of Edinburgh. Two Volumes. New York, Scribners; Edinburgh, Blackwoods. First Series. 1895. Second Series. 1896.

These two volumes should be read and reviewed as a single work. Indeed, in the preface to the second volume, Professor Fraser expressly urges that the two volumes 'be looked at as a continuous inquiry, not as a series of isolated discussions.' Taken thus as a whole, these Gifford Lectures are a singularly interesting expression of a long life of scholarship and faith. For sixty years, Professor Fraser, as student and teacher, has been associated with his university. He succeeded Sir W. Hamilton as long ago as 1859, and now, in his ripe old age, he offers this testimony to the reasonableness of religion. His volumes do not pretend to maintain new points of view or to enter far into philosophical controversy. They are, as he says: "An honest exposition of results already reached in a life devoted to similar pursuits." No one can thus receive from this veteran, well known through his earlier studies of Berkeley and of Locke, this final *Apologia pro fide sua* without grateful reverence. It is a book which invites not so much criticism as appreciation; and it will be sufficient to describe its methods and its conclusions.

In the first volume, as Professor Fraser remarks, "the voice of the sceptic was prominent; in the second, faith makes itself heard." At the outset, and with great gravity and dignity, he defines the 'final problem' of all thought. "Is religion an intellectually legitimate state of mind?" "What sort of a universe is this in which I find myself?"; and he 'articulates' this ultimate problem as holding three factors of universal experience—the material world, the subjective self and the spiritual reality of God. He is then led on to consider the three forms of monism thus suggested—the life of God and man interpreted through materialism; the life of God and matter inter-

preted in terms of subjective idealism; and the life of matter and mind as absorbed in Deity. In the first case he concludes that even reason itself, and its product, science, are lost in the flux of things, and even "materialism itself disappears in the abyss of universal nescience." In the second case, while affirming the superior claim of idealism, he urges its strictly individual limitation. "Individual Egoism is eternally confined in the individual Ego." The third possibility, that of Pantheism, detains him longer, as a faith "which has brought peace to millions of human minds;" and his discussion of this faith is the most weighty section of his first volume. Finally, there remains to be negatively criticized, the attitude of absolute scepticism; and here he reviews the agnosticism which, he believes, should be identified not with modern men of science but with Hume.

Science itself, Professor Frazer concludes, is finally an act of faith, not of reasoning, and the "agnosticism that retains science is not really a protest against faith; it is only an arrest of faith."

At this point in the first volume begins the positive treatment which the second volume completes. A spiritual interpretation of the universe, he goes on to affirm, is, at least, not inconsistent with the facts of Nature, but it is disclosed with far greater fulness by the self-consciousness of man. Man as a moral being brings us into relation with the supernatural. Cosmic faith is the assurance that the natural world will not put us to intellectual confusion; moral faith is the assurance that those who strive for goodness shall not be put to permanent moral confusion. Many mysteries of the universe remain unexplored, but its fundamental character is indicated by the spiritual life of man. *Homo mensura*. The second volume studies, in greater detail, this moral and spiritual man in the midst of a moral and spiritual universe. The working postulate of human life is found in this assumption of a spiritual relation to reality. This moral faith alone permits us to interpret the causation and the design of nature. "The presence, throughout the whole, of latent meaning and moral purpose is not indeed a conclusion that can be logically drawn from the few physical or moral phenomena that are actually offered to us in our experience; but the assumption is warranted * * * * * * as the needed condition of our escape from speechless and motionless Pyrrhomist despair." Finally, there is dealt with the special fact which seems most flatly to contradict this impression of spiritual design. "How can a universe of suffering and sin be a revelation of omnipotent goodness?" To this final difficulty the two-fold answer is given; that it is a world of moral discipline and

education, and that its full interpretation is postponed to another life. Very soberly and with much reserve the mystery of death is faced. "Faith in the persistence of morally responsible persons is not the indispensable postulate of all reliable intercourse with the universe of things and persons; but its disintegration would disturb the theistic trust and so leads to universal doubt." "It is the irrational alternative in the dilemma that makes optimistic trust the highest philosophy."

Such is this veteran's philosophic faith: "the natural trust that nothing can happen in the temporal evolution which can finally put to confusion the principles of moral reason that are latent in man."

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A History of European Thought in the Nineteenth Century. JOHN THEODORE MERZ. Vol. I, Introduction; Scientific Thought, Part 1. Edinburgh & London, William Blackwood & Sons. 1896. Post 8vo. Pp. xiv+458.

Alike in science and philosophy it has ever been a marked characteristic of English thought to be served by independent as well as by professional workers. Priestley, Davy, Wollaston, Young, Dalton, Faraday and Joule were, like Bacon, Locke, Berkeley, Bentham, the Mills, Grote, Buckle and Mr. Spencer, outside the pale of the universities. Among this distinguished company Mr. Merz (long known favorably to a limited circle of the cultivated at Newcastle-on-Tyne, and to the philosophical specialist as the author of the excellent little monograph on *Leibniz* in Blackwood's philosophical series) now takes his place by right of achievement. Should he continue his *History* to completion as he has begun, he will rank not far from the most eminent of the non-academic writers.

At the present juncture criticism would be beside the mark, and it may serve meantime to call the attention of thinkers to the plan and execution of this most valuable book, to preparation for which Mr. Merz has given no less than thirty years. The author speaks in his preface of the encouragement derived from friends during his long period of self-suppression; he will now be borne up through the rest of his self-imposed task by the universal expectation of all who take real interest in either scientific or philosophical pursuits.

The present volume consists, *first*, of an Introduction, in which three main points are discussed;—the necessity for such an undertaking and its timeliness; the reasons for confining attention to European culture, especially as wrought out by the three great nationalities of

Britain, Germany and France, and the general groundwork of the entire undertaking, which may eventually extend to four or five volumes. The last alone need trouble us now. Mr. Merz' general standpoint may be gleaned from the following: "Unless I believed that our age was elaborating a deeper and more significant conception of this unity of all human interests, of the inner mental life of man and mankind, I do not think I should have deemed it worth while to write the following volumes, for it is really their main end and principal object to trace the coöperation of many agencies in the higher work of our century; the growing conviction that all mental efforts combine together to produce and uphold the ideal possession of the race; that it is not in one special direction nor under one specific term that this treasure can be cultivated, but that individuals and peoples in their combined international life exhibit and perpetuate it." (p. 33.) The plan of the work, as outlined at p. 63 sq., is to treat first of science—'thought as a means to an end.' In the second part, "we have to consider it as its own object, as a reflection on itself, carried on with the object of knowing its own origin, its laws, its validity. This discipline may, as a whole, be called philosophy." So far as I am able to gather from Mr. Merz' statement, two volumes are to be devoted to the first section; a volume to the second, and another to the 'unmethodical thought,' which he groups under the name of religion. Following this scheme, the *second* part of the present volume contains three introductory chapters on the Scientific Spirit in France, Germany and England, respectively; and two systematic chapters, one on the Astronomical View of Nature, the other on the Atomic View of Nature. It will be of interest to readers of this REVIEW to learn that the second volume will present a similar conspectus of the Psychological View.

I can only add that, in my judgment, this work bids fair to rank as a classic. It is suggestive, its learning is admirably balanced and unified and its objectivity is wholly unusual. Everyone ought to obtain it as an indispensable *vade mecum*.

R. M. WENLEY.

UNIVERSITY OF MICHIGAN.

Gustav Theodor Fechner. KURD LASSWITZ. Frommanns Klassiker der Philosophie, edited by RICHARD FALCKENBERG. No. I. Stuttgart, Frommanns Verlag. 1896. Pp. 207.

The author presents, not only the life and writings of Fechner, but also his view of the world. He believes the study of this view

will be helpful to present and future thought. The presentation is condensed and systematic; the style clear and touched all through by an enthusiastic admiration of the man and his work. The book may be said to 'read itself,' and one comes to feel that Fechner was both a hero and a genius as a result of this glowing treatment.

The subject is divided into two parts: (1) Life and Works, and (2) View of the World. The first part is divided into three periods. The work of confirming Ohm's law and extending its application to the many possible variations of conditions was Fechner's. As the author of the 'Comparative Anatomy of Angels' and other similar pieces he ranks high as a humorist and a lover of belles-lettres. His work in psycho-physics, experimental psychology and æsthetics is well known and of acknowledged permanent value. Fechner's life is made interesting by the author's accounts of his struggles with poverty, sickness and defective eyesight, as well as by his scientific successes.

Fechner's view of the world is presented in two parts: (1) the Theory of Motion and (2) that of Consciousness. The ingenious and mathematically beautiful effort of the philosopher to state the phenomena of the material universe in terms of the motion of a system of things impresses one as rich with suggestion. The most interesting point is the demonstration that the law of attraction which, when stated as it usually is, in terms of force, fails to explain the phenomena of cohesion and chemical affinity, stands, when stated by Fechner in terms of motion, in perfect accord with these phenomena. This enables him to extend the law to the organic realm, and he conceives the earth itself to be simply a universal organism.

As to consciousness, the physical and the psychical are identical, not two aspects of the same thing or causes the one of the other; they are the same thing looked at from two different points of view, just as the same curve is convex from without and concave from within. There is but one consciousness, as there is but one earth, and individual minds are simply points at which the universal divine consciousness 'crops out,' the 'threshold' of its appearance being a certain degree in the complexity of the individual organism. His view of attention is psychologically interesting. The principle of stability seen in the external world is the principle of the tendency to harmony in the inner—"the world strives toward a maximum of pleasure" for the universal, divine consciousness. His ethic is hedonistic but not altogether empirical, for its highest good is the pleasure of a consciousness which transcends that of man. The will is determined from within by its own nature, and is therefore free. Man is "a part of the divine con-

sciousness and, between men, the World is not dark and dumb, rather she sees and hears with the mind of God."

G. A. TAWNEY.

BELOIT COLLEGE.

Habit and Instinct. C. LLOYD MORGAN. New York and London, Edward Arnold. 1896. 8vo. Pp. 351. $4.00.

Principal Morgan is known as the author of two excellent books, and his new volume will go far towards increasing his reputation as an interesting and original writer. He is a disciple of Darwin, a pupil of Huxley, and a follower and friend of Romanes. His book bears clearly the traces of these three men's influence; it is written from the evolutionary standpoint, it is based on the observations of a naturalist, and its theme is psychological. A considerable body of new observations, of entirely fresh facts are presented. The immediate object of the author was to ascertain for some of the higher animals the limits of instinct. He accordingly devoted himself to a prolonged and patient study by observation and experiment on the ways of young mammals and birds, chiefly the latter, as offering a more favorable field, since they can be readily reared away from the parents.

The main result of the book may be indicated by saying that the author demonstrates the predominating control of experience and its educational power during the young life in fixing adult habits. It is the detailed, keenly analysed evidence of this which imparts to the volume its chief value and originality, and renders it very refreshing after the great mass of uncritical writings on animal psychology. Of wider interest are the concluding chapters, which are devoted to a broad discussion of the admissibility of the Neo-Lamarckian doctrines in the author's field of evolutionary enquiry. The distinction which Morgan draws between modification and variation is a welcome addition to clearness of thought, but the coöperation between the two, which he seeks to establish with a view of making a compromise between the theory of the Neo-Lamarckians and that of Darwin, can be regarded as hardly more than an offer to capitulate the Lamarckian position if the honors of war are allowed.

With the material which he had on hand the author could have written a paper, which, published in some psychological journal, would have made his work known to his professional colleagues in a very much briefer form. He has chosen, on the contrary, a more popular method, and has based his book upon a course of lectures delivered before the Lowell Institute in Boston. He has thus become

more readable, and ought to reach a wider public, though to the scientific reader the sacrifice is considerable. The author's presentation of his subject is excellent and his style very clear, though his habitual use of 'would seem' when he means 'seem' grates on the reader's ear. In the numerous critical passages he is calmly impersonal, although much that has been written on animal psychology might easily call forth sarcastic criticism. It may not be amiss to add that the book is made pleasanter to read by the occasional cropping out of a vein of quiet humor.

The work must be recommended very warmly to psychologists and ornithologists. The latter are probably past redemption, for an ornithologist is most rarely a bird-lover and seldom does the ornithological mind conceive a bird as more than a species with a Latin trinomial. To psychologists this book should bring much. As a naturalist, it has seemed to me that the naturalist's method has an immense future in psychology. The method includes two main factors: the observation of details and the comparison of homologous phenomena in different forms of life; and the method starts always from the standpoint of evolution. There need be no restriction, of course, upon the three aspects of psychology, which have heretofore prevailed, the metaphysical, introspective and experimental, but there should come soon and with revolutionary power, not merely enlarged interest in and sympathy with comparative evolutional psychology, but more than that, eagerness to enter this field of inquiry and to share in harvesting it. Those who follow the new trend can hardly begin better than by making acquaintance with Principal Morgan's recent volume, which illustrates how to begin and shows that there are substantial rewards for those who will investigate soberly and scientifically the mighty problems of psychological homologies.

CHARLES S. MINOT.

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SOCIAL PSYCHOLOGY.

The Crowd: A Study of the Popular Mind. GUSTAVE LE BON. New York, The Macmillan Company, 1896. Pp. xxiv+320.

We call attention to this (slightly clumsy) translation of a book already noticed in these pages, because, in spite of its grave defects, it is almost the first scientific attempt to treat a subject of supreme importance, and ought to be read by everyone who is interested in the problems which popular government presents. With public opinion

ruling more and more the world, the psychology of public opinion, the sources of its strength and weakness, its pathology and hygiene, and the causes of its stability and of its alterations, ought to be studied with ever-increasing care by those interested in the welfare of mankind. It can hardly be doubted that M. Le Bon's little books will communicate a serious stimulus to study of this sort. He is a many-sided person, physiologist, anthropologist, traveller and historian, and knows the outside world as few Frenchmen know it; but of human life he takes a purely biological view, devoid of every sentimental or metaphysical ideal, and his results are misanthropic and pessimistic in the extreme. Man's worst enemy, it would appear, is man himself; for whilst nothing great can be done except by men in concert, it happens that all the feelings that move men together, all the patriotic, religious and philanthropic ideals which they will obey in crowds and collections, are more or less irrational and insane. Rationality indeed would, on M. Le Bon's view, seem necessarily confined to the isolated man of science or individual critic, and the only ideal permissible to *him* would be that of sound skin and safety from physical danger, for himself in the first place, and thenceforth for as many fellow-creatures as circumstances allow to be embraced. Anglomania is the practical result of all this—a result shared by the entire school of Frenchmen who follow the lead of Taine. For in England and America, whatever minor inferiorities these countries may show, the individual is left more to himself and his affairs are on the whole more safe. France, M. Le Bon seems to fear, is rushing blindly to a destiny which, being the outcome of ineradicable ideals of equality and centralization, will hardly stop short of complete socialism, with everyone at once a slave and a pensioner of the State.

It is curious for us Americans, who are just beginning to idealize less our national ways, to hear them so much idealized by foreigners. Meanwhile, in a sense, it is, of course, true that man's worst enemy is himself; the worst enemies of some ideals are other ideals, and men in crowds, even though those crowds be called deliberative assemblies, are often terribly unwise. But it is only from the standpoint of some one ideal held for true that all other ideals can be tried and condemned, and the great trouble with books like M. Le Bon's is that whilst they are inspired by very distinct ideals these are not expressed in them in frank teleological form. To a reader even half-respectful of the socialistic ideals of the present generation, it seems rather a *reductio ad absurdum* of the pretension of Science to look down upon all such ways of thinking as essentially crazy, to find that her own last word of prac-

tical wisdom about human life is to advise her votaries to dwell on the frontier and have bonds invested in many countries, so that when that insane beast Man 'breaks out' in one they may get into another escape. M. Le Bon does not give this advice in so many words, but it exhales somehow from his pages; and, *Lebensweisheit* for *Lebensweisheit*, it hardly seems obvious that, in comparison with this last rational outcome of Science, the religions and the philanthropies, with all their tendency to insanity, make such an inferior show.

Let it not be thought that our author's contempt for religious and other mob-swaying ideals, and his respect for Science, involve any snobbish deference to 'scientists' as a caste. He is, on the contrary, singularly free from all caste deference. Witness his defence of juries, to the irrationality of whose opinions in criminal cases he has devoted a chapter:

"Many writers, some of them most distinguished, have started of late a strong campaign against the institution of the jury, although it is the only protection we have against the errors, really very frequent, of a caste that is under no control. A portion of these writers advocate a jury recruited solely from the ranks of the enlightened classes, but we have already proved that even in this case the verdicts would be identical with those returned under the present system. Other writers, taking their stand on the errors committed by juries, would abolish the jury and have it replaced by Judges. * * * We should cling vigorously to the jury. It constitutes, perhaps, the only category of crowd that cannot be replaced by any individuality. It alone can temper the severity of the law which, equal for all, is bound on principle to be blind and to take no cognizance of particular cases. Inaccessible to pity, and heeding nothing but the text of the law, the Judge, in his professional severity, would visit with the same penalty the burglar guilty of murder and the girl * * * driven to infanticide. * * * Being well acquainted with the psychology of castes and also with the psychology of other species of crowds, I do not know a single case in which, wrongly accused of a crime, I should not prefer to have to deal with a jury rather than with magistrates. There would be some chance that my innocence would be recognized by the former but not the slightest chance that it would be admitted by the latter. The power of crowds is to be dreaded, but the power of certain castes is to be dreaded yet more. Crowds are open to conviction; castes never are" (p. 188).

In future books of this sort the objective psychology and the subjective teleology of the author will have to be kept more distinct, and the latter made more explicit. But the present volume, with all its

faults, is a most honest and vigorous production which should be widely read.

W. J.

Psychologie du Socialisme. G. Le Bon. Rev. Philos. December, 1896.

The author proposes to apply the principles developed in his previous works to the phenomena of 'socialism,' comprising under this term "the aspirations, wants, beliefs, ideas and reforms which are to-day the profound passion of many minds." To appreciate the power of socialism we must consider it not from the political or economic standpoint, but as a *belief*, and the student of the psychology of beliefs will understand why argument is quite in vain against the collective belief of a multitude.

In its fundamental principles socialism is but a repetition of tendencies which emerged in Hebrew, Greek and Roman life, and reached an actual triumph in early Christianity, only to be abandoned when Christianity became itself a conservative institution at present, although the actual condition of the poorer classes is much superior to that of former times, yet their wants and desires have increased so rapidly that the ratio is more on the side prompting to discontent than ever before. Add to this the prevalent egoism, demoralizing devotion to wealth and indifference of the ruling classes, the pessimism of thinkers, the half-heartedness of the defenders of the present social order, comparable to the weakness of the defenders of dying paganism, and, above all, the decay of the great dominant beliefs of the past which leaves men ready and eager for some new and inspiring belief, and the marvel is not that a new religion like socialism progresses so fast but that it does progress faster. To understand this we must recur to the psychological laws of the evolution of beliefs.

Man is guided in life by two classes of conceptions, *ancestral conceptions*, or *sentiments* on the one hand, and *acquired* or *intellectual conceptions* on the other. The former are the great motives in conduct. They are the atavistic influence to which is due the real conservatism of crowds, often masked as this may be by temporary agitations. The acquired or intellectual conceptions remain almost without influence upon actual conduct until, by repeated hereditary accumulation, they have penetrated the depths of the unconscious and become sentiments. Buddhism, Christianity, Islam were no new faiths. Christianity triumphed not because it was new, but because it embodied the sentiments of Greek and Egyptian and Persian as well

as of Jew. At the very beginning a belief may have roots in the intelligence, but when it becomes the actual motor it becomes rather the regulator of the intelligence, the touch-stone of judgment. The mind can receive only what conforms to the new belief. Philosophy, literature and the arts all receive its impress, as in the middle ages, or among the Arabs. All new conceptions and perceptions must be unconsciously shaped by these ancestral conceptions before they can gain entrance to the mind. This is strikingly illustrated in the changing meanings attached to words from age to age, or as used by different races and sexes, a suggestive subject for psychological investigation. On technical subjects there may be intellectual discussion and agreement. But in morals, politics and religion agreement, or even mutual understanding is possible only for those of a common origin. In conference on these themes "it is not the living but the dead who discuss." Ordinary psychology assumes that all men experience identical sentiments under the influence of like stimulation, but nothing is more erroneous. A common well-rooted belief becomes thus the source of common ideas and the greatest factor in the creation of a national soul and will, and so of a characteristic orientation for all ideas.

The psychology of the apostles of the new faith is instructive. Although alienists usually regard the advanced socialists as belonging to the 'criminal-born' type, this is quite a mistake. They are rather actuated, not as the true criminal, by selfishness, but by motives the reverse of selfish, leading to acts quite opposed to their own interests. Like the apostles of past faiths, the men of the Inquisition, the followers of Mahomet, the men of the Convention, their zeal prompts them to destroy first institutions, then all who resist. Their philanthropy is as sincere and as intolerant of opposition as that of former apostles. They may be regarded as hypnotized by two or three formulas constantly brooded over.

Sociology and Philosophy. B. Bosanquet. Mind. January, 1897.

Sociology is coming to be regarded more and more from the point of view of social psychology, the point of view of Plato's *Republic*. This enables us to see its relation to social philosophy. Like individual psychology it is now interested in asserting its claim to be a natural science, and as such to treat all phenomena in its field impartially. The 'laws of association' are the object of investigation, without regard to the logical, ethical or social *value* of the product.

But just as in the concept of apperception we recognize that the mind is not merely a machine, unaffected by its content, but is very different according to the reality which it thinks of, so, in social psychology, when it has ceased to be necessary or worth while to direct our attention solely to the fact that there is a common element in all social groups, we shall inquire into the differences as well. From this the transition to the consideration of the relative social value of the different groups or forms of organization is an easy one, and this is the province of social philosophy. The service of social science to social philosophy may then be similar to that of psychology to logic, ethics and æsthetics, but it would be for the advantage of the sociologist to familiarize himself far more intimately than he has yet done with psychological conceptions and principles, for which he now seems to be vaguely groping.

J. H. TUFTS.

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NEUROLOGY AND PHYSIOLOGY.

The Cell in Development and Inheritance. EDMUND B. WILSON. Columbia University Biological Series, IV. New York and London, The Macmillan Co., 1896. Pp. xvi+371

Within recent years the problems of biology have centered more and more largely in the cell. As long as morphologists were chiefly concerned with the evidences and probable course of organic evolution interest gathered around questions of homologies of organs and affinities of organisms, but now that the paramount problem is the *cause* of evolution the old methods are generally found to be of little service. What light could homology or phylogeny throw upon the nature and causes of assimilation, growth, metabolism, inheritance or development? And yet upon these very questions hangs the causal explanation of vital phenomena in general, including evolution. In almost every case these problems have been found to be at bottom questions as to the structure and function of the cell, and in attacking them morphology has of necessity become physiological. In the life of the cell are centered most of the present philosophical problems of biology.

The appearance, therefore, at this time of a general work on the cell is of more than ordinary interest, not alone to the biologist, but to all persons of liberal ideas. Professor Wilson's work is not the first in the field, though I think it may be said to be easily the best. Hert-

wig's splendid treatise, *Die Zelle und die Gewebe* (1893), was really the pioneer volume in this field; it treats the subject in a more general way than Wilson's work, taking in the non-developmental as well as the developmental cell phenomena, but it is, of course, far less rich in references to recent important literature. The only other work which deserves to be compared with Wilson's is Henneguy's *Leçons sur la Cellule* (1896). This volume is in typography and illustration a work of art, but, like so many other modern French works on biology, it is in large part a huge, encyclopedic compilation and is too technical and diffuse for the general reader.

Professor Wilson has wisely limited his work to the developmental cell phenomena in which at present knowledge is most advanced and interest most intense. After an introduction which gives a brief historical sketch of the cell theory and its relation to the evolution theory, there are taken up, in successive chapters, a general sketch of cell structure, cell division, the germ cells, fertilization, chromatic reduction, some problems of cell organization, cell chemistry and cell physiology, cell division and development, and finally some theories of inheritance and development. In addition there is appended an excellent glossary and a general list of literature.

The book contains a large amount of Professor Wilson's own work and that of his pupils, some of which has not heretofore been published, but such a general work must of necessity be to a large extent founded upon the work of others. The author has brought together, under one point of view, the isolated observations and frequently conflicting views of a multitude of writers. In this he has graciously and entirely avoided the old museum idea of collecting material without reference to its use; although he summarizes almost every important work of recent years bearing either directly or indirectly upon the cell, yet the book is no mere encyclopedia of facts or theories—all is treated in a critical spirit as so much material to be builded into a system. The labor involved in this sifting of literature and collation of results must have been prodigious and all future workers in these lines will owe Professor Wilson a debt of gratitude for the service which he has thus rendered.

The limits of this notice will not allow a review of the conclusions of the author on the many subjects discussed. Those, however, which are of the most interest to the general reader are contained in the final chapter of the book and may receive brief mention here.

The author indicates that all present discussions of development revolve around two hypotheses, both of which are regarded as em-

bodying fundamental truths. The first is the *Germinal Localization* hypothesis of His, the second the *Idioplasm* hypothesis of Nägeli; the former asserts that the cytoplasm of the egg contains in definitely localized areas the germs of future organs (*'organbildende Keimbezirke'*); the latter regards inheritance as the result of the molecular organization of a peculiar kind of living matter, the *idioplasm*, which is now generally identified with the chromatic substance of the nucleus. With regard to the application of these principles to development there are two widely different views. The Roux-Weismann theory of development holds that cytoplasmic differentiation is due to nuclear differentiation and that the latter arises from qualitative divisions of the nuclear substance. The opposing view of Driesch, Hertwig and others is that divisions of the nucleus are always quantitative, never qualitative, and that the progressive differentiation of the cytoplasm is the result, not of the progressive differentiation of the idioplasm, as Roux and Weismann hold, but of the relative position of cells with reference to each other. More recently both Driesch and Hertwig have been compelled to supplement this view by granting that chemical and physical differences exist in different regions of the egg-cytoplasm and that subsequent differentiations arise through the reaction of these different substances upon the nuclear idioplasm which constantly remains the same. With the main points of this hypothesis Professor Wilson is in hearty accord; however, he adds this further very important conception that the specification of the cytoplasm, induced by the nucleus, reacts upon the latter, bringing about a specification of the idioplasm, so that in the end there is a differentiation of nuclear material, though not brought about by qualitative divisions.

The specific character of the development with its orderly course of events is regarded, as in almost every modern theory of heredity, as the result of the *structure* of the idioplasm. The nature of this structure, as Professor Wilson points out, involves the old controversy of preformation and epigenesis, "a controversy which now has little meaning apart from the general problem of physical causality. Despite all our theories we no more know how the properties of the idioplasm involve the properties of the adult body than we know how the properties of hydrogen and oxygen involve those of water." The problem of the historical origin of the idioplasm 'is merely the problem of evolution stated from the standpoint of the cell.' "Whether variations first arise in the idioplasm, as Weismann maintains, or whether they may arise in the body cells and then be reflected back

upon the idioplasm, is a question upon which the study of the cell has not thus far thrown a ray of light."

Finally, Professor Wilson considers the nature and origin of coördinated *fitness* as the fundamental problem of biology. In this regard there is an enormous gap between the lowest forms of life and the inorganic world which the study of the cell has only served to widen. With Wigand and Driesch the author thus, apparently, returns to a form of vitalism which, for the present at least, seems the only justifiable course.

The book is written in good literary English, an unusual thing in recent biological works, and, although dealing with some very abstruse and obscure themes, it is remarkably clear and logical throughout. Professor Wilson's style is that of a teacher at his best, precise, concise, enthusiastic. In typography and illustration the volume is a model of excellence; many of the figures are entirely new and few of them have ever before appeared in a general work of this character.

Considering the great amount of ground covered there are remarkably few errors in the volume. Perhaps the most important one is as to Van Beneden's view on the origin of the centrosomes of the first cleavage spindle (pp. 156–7). This Professor Wilson has already corrected (*Science*, Jan. 1, '97). Another error, of minor importance, however, is the statement (p. 113) that the amœboid egg cells of Cœlenterates probably do not ingulf other cells.

To me it seems that the feature which is most open to serious criticism is one which gives the work one of its particular charms, *viz*, its enthusiasm and, in some places, its controversial spirit. Professor Wilson frequently uses strong language, sometimes stronger than seems to be justified. He speaks of a certain abandoned theory as having been 'absolutely proved to be a myth.' He says the Roux-Weismann hypothesis of qualitative nuclear divisions "is as complete an *a priori* assumption as any that the history of scholasticism can show and every fact opposed to it has been met by equally baseless subsidiary hypotheses." Examined in the light of the facts "the improbability of the hypothesis becomes so great that it loses all semblance to reality" (p. 307). And yet a few pages further on (p. 321) he strongly supports the most fundamental part of the Roux-Weismann theory, *viz.*, that *nuclei progressively become qualitatively different throughout the development.* Since the only point of difference, therefore, between Professor Wilson and the Roux-Weismann theory is as to the method by which these qualitative differences of the nuclei arise, such strong statements as those just quoted scarcely seem justified.

Again, in the matter of the origin and significance of the centrosomes in fertilization he goes much farther, I think, than the facts warrant. It is true that in the majority of known cases the centrosomes of the first cleavage spindle come from the spermatozoon, and Professor Wilson has been most active in establishing this fact, but there are cases in which these centrosomes are known to come from the ovum, and, until we know whether the centrosome is really a permanent cell organ or not, it is too soon to assert that the point of origin of the centrosome is a matter of primary significance or that "the centrosome is the fertilizing element proper."

These are, however, criticisms of minor significance. As a whole the work is a remarkably able and comprehensive presentation of the most important biological problems of the day and it easily takes first rank among books of its class.

E. G. Conklin.

University of Pennsylvania.

Atlas of Nerve Cells. M. Allen Starr, M.D., Ph.D., With the coöperation of Oliver S. Strong, A.M., Ph.D., and Edward Leaming, M.D., F.R.P.S. With fifty-three plates and thirteen diagrams. New York, Columbia University Press, Macmillan, 1896. Pp. 78.

The revolution in methods of nervous histology brought about by Golgi's discovery of the possibility of defining nerve cells and processes by a precipitation of metallic salts about the elements has been followed by an activity in research in that field which has hardly a scientific parallel. As might be expected, we are now reaping the results and without regarding the rather startling speculations as to cell function which have made their appearance during the last twelvemonth, the fairly well established and probably valid conclusions arrived at are of great interest and importance. We are no longer to consider the central nervous system as made up of nerve cells and nerve fibres but of units, each consisting of a nerve cell with its processes, of which one is greatly prolonged and is our old nerve fibre while the shorter cell processes which were formerly delegated to the uninteresting duty of nutrition are now elevated to the dignity of auxiliary receptive functional parts. We no longer have our cell with its axis cylinder and protoplasmic processes but have a 'neuron' as our unit, made up of a cell with its 'neuraxon' and its 'dendrites.'

More interesting still is the demonstration of lack of continuity between these units, the nervous impulse traversing the series of neu-

rons by contiguity. This anatomical independence, while perhaps not proven beyond cavil, may at least be accepted as highly probable. These facts together with the demonstration of the collateral branches of the neuraxons are the essentials of the discoveries of the last few years and need only be mentioned to obtain recognition of their importance as furnishing a scientific basis for physiological and pathological theory.

In the superb atlas before us, Dr. Starr has undertaken to show by photographs the state of things at the present day. It may as well be said at once that, considering the methods used, little criticism can be offered. Dr. Strong, who is responsible for the preparations, has attained a perfection of technique that leaves nothing to be desired, Dr. Leaming's mastery of micro-photography is well known and Dr. Starr's explanatory text is characteristically clear and comparatively conservative. The shortcomings of the work are the limitations of the methods. No photograph can show more than one level of a section and it can focus but a small area and as a consequence much that is strikingly evident upon a single turn of the adjustment of the microscope must be taken on faith. In other words, one good slide under the microscope is worth all the photographs in the world in acquiring an idea of the object. This is, of course, a difficulty inherent in the means and the authors have been very judicious in their choice of levels of the sections which would minimize the disadvantage. Further, the Golgi method, valuable as it is, is gross. It blots out details of cell structure in the most ruthless way and we are still in need of a cell stain for finer work on which to base functional conclusions. The object of the atlas is, of course, mainly anatomical; at the same time Dr. Starr's text does not neglect the physiological aspect of the question and there are numerous digressions on that side which deserve notice. Particular attention is called to the brief discussion of the mechanism of reflex action in the cord on pp. 24 and 25, in connection with the function of 'collaterals' and the length of the reflex arc which may extend and, as a matter of fact, probably normally does extend several segments up and down the cord from the point of entrance of the sensory impulse. This is the clearest statement of this phase of reflex action which has come to the notice of the writer and it is a phase too much neglected by contemporary writers on general physiology. To sum up, the work is good in all its aspects and the obvious question whether it is all worth while is not to be answered by any individual. It is not so much an original contribution as a resumé of progress already made and, as indicated above, so far as that can

be shown by photography it is admirably done. As a specimen of book making the atlas is magnificent, but the price is prohibitive for students and probably for many laboratories. It would be a great service if the plates could be reproduced by a cheaper process and sold separately to aid class room demonstration.

LIVINGSTON FARRAND.

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VISION.

Ueber die functionellen Verschiedenheiten des Netzhaut-Centrums und der Nachbartheile. PROFESSOR v. KRIES. Archiv für Ophthalmologie, XLII., (3), 95–133.

Professor v. Kries here replies to Koster (see this REVIEW, Vol. III., p. 108 and p. 231) who doubts the conclusiveness of the considerations which go to show that the cones are the seat of color-sensation, while the rods furnish us with sensations of gray of different degrees of brightness; he makes a very strong showing in favor of the thesis which he defends, and his able summing up of all the evidence can hardly fail to carry conviction.

V. Kries finds that the Purkinje phenomenon (the increased relative brightness of blue in a faint light) does not occur in the fovea; that Koster differs with him on this point is plainly owing to the fact that he is unaware of the extreme difficulty of looking at anything with the fovea when the light is faint,—there is an almost irresistible pull in favor of using an adjoining part of the retina on account of its superior efficiency. The strongest of all the arguments is that two grays composed of different complementary colors, if made to look equally bright at an ordinary illumination, no longer look so in a faint light,—it is difficult to believe that only a single apparatus is involved in furnishing the sensation at the two extremes, if adaptation works so differently upon lights of different composition; when both terms of the comparison are of the same quality, gray, the difficulty which is always felt in comparing different light-qualities in respect to their intensity does not arise, and hence the experiment in this case is of a peculiarly convincing nature. Recurrent vision v. Kries has already attributed to a distinct functioning of the rods, and he finds now that a patient who was suffering from night blindness (which had already been made out by Parinaud, in 1883, to be due to a lack of visual purple) wholly failed to get the recurrent image. Koster found a discrepancy between the extent of the fovea and the

area of the space within which adaptation does not occur; v. Kries has now measured the functional fovea (as this latter retinal space may be called) by several different methods and finds an extremely good agreement with the size of the anatomical fovea as lately determined by Koster. The diameter of the functional fovea, when projected to a distance of one meter from the observer, was found by the different methods to be:

By Purkinje phenomenon, ...	59 mm.
By same for two dichromates, ...	35 mm.
By recurrent vision, ...	36 mm.
By same for another observer, ...	52 mm.

Koster's anatomical determination gives for the rodless space 33 mm. and for the space within which the rods are very few 53 mm. The coincidence is, therefore, very close; it is also possible that a better method may yet be devised for the determination of the functional defective area.

C. L. FRANKLIN.

BALTIMORE.

Über kompensatorische Raddrehungen der Augen. W. A. NAGEL. Zeitschr. f. Psych. u. Physiol. d. Sinnesorg. XII., 331–354. 1896.

When the head is inclined sideways, do the eyes rotate about the sagittal axis in the opposite direction, or do they retain their normal positions in the head without rotation? This question has been argued much on both sides. It had been practically settled in favor of rotation until 1894, when Contejean and Delmas again disputed it. Nagel again proves the existence of rotation by several methods. It can be seen directly on the eye of another person, or on the reflection of one's own eye in a properly arranged mirror, by observing the position of the radial lines of the iris. Care must be taken to prevent change in the direction of the line of regard, that there may be rotation only about it. The ray-like figure, due to irregular astigmatism, seen proceeding from distant luminous points, will also demonstrate the rotation if the direction of its rays, which show the eye's position, be compared with lines on a spectacle-lens whose position is fixed relatively to the head. A third method makes use of after-images. The denial of rotation above mentioned was based on measurement of the position of the blind spot, determined by suitable diagrams for different positions of the head. Nagel uses this same method for quantitative measurements. When the head is inclined 20°, he finds $\frac{1}{3}$ of

this compensated by rotation; the proportion grows gradually less up to 80°, where it is $\frac{1}{10}$.

Experiments on animals show similar movements of rotation in many cases. Animals whose eyes are placed in the side of the head and have no common field of regard show rotation when the head is inclined not sideways but in the sagittal plane. It varies in degree from full compensation in case of guinea pigs to its entire absence in some birds, where it is replaced by compensatory head movements. The mechanism for initiating these movements of rotation is situated in the labyrinth.

E. B. DELABARRE.
BROWN UNIVERSITY.

EXPERIMENTAL.

Die motorischen Wortvorstellungen. RAYMOND DODGE. Halle, Niemayer. 1896. Pp. 78.

Mr. Dodge gives an admirable introspective study of his own verbal imagery, analyzing it in all its varieties. Silent thinking in words is for him mainly an inner speaking. But reproduced articulatory images are not the only element. Connected with each word it is possible to represent vaguely the essence of all that the whole sentence is intended to express. Although he can never represent clearly two words at once, or even a single word in all its parts, yet the shadowy sentence-image is made up of many simultaneous verbal images of lesser clearness, and is not identical with the speechless meaning or concept, which the full sentence expresses. The characteristic elements of the words themselves are reproductions of the movement-feeling which arises in actual speaking; derived mostly from lips, tongue and throat, less clearly and characteristically from breast and thorax. Contact and vibration sensations are present, but not essential, as was shown for the former by producing through cocain a strong anæsthesia of lips, tongue and throat. Stricker's assertion, that no sensory elements are present in his motor verbal images, and that they consist in innervation-images, is shown to be indefensible. It is impossible that sensory motor images can be entirely lacking, whether innervation-feelings exist or not; and D. can find by introspection no trace of the latter. Peripherally aroused sensations from actual articulatory movements are not essential for inner speech; their reproduced images suffice. When present, they raise the mental presentation of words to greater clearness. Unusual and incompatible positions of the mouth disturb the mental presentation of a sound for

a moment only; and show not that particular peripheral sensations must be present, but that strong peripheral sensations of another kind may inhibit for a moment the desired verbal image.

In addition to the reproductions of motor sensations, there must be other elements in the verbal image. For articulatory movements alone, unpreceded by the idea of expressing a word, do not necessarily awaken a verbal idea. The latter possesses a filling, a fullness which can be given only by a sort of unlocalized, faded-out auditory imagery, which never attains an independent, clear and recognizable reproduction. No trace of visual elements can be detected in any recognizable characteristic of the word-image. It is shown, however, that they must be aroused to a certain extent, unconsciously influencing and controlling the conscious imagery. Images derived from writing movements cannot be detected or inferred, unless when a word is spelled. In short, motor images are prominent, and are recognizable as such; auditory images are not recognizable as such, but furnish a recognizable portion of the content; visual images cannot be detected in any conscious feature of the content, but their presence is evidenced by their control.

Similar thorough analyses, with inferences as to the brain-paths used, are given of the verbal elements present, prominently or vaguely, in speaking aloud, in hearing, in reading and in writing. Whichever one of the four kinds of word elements is most prominently aroused, its firm association with the others arouses them also, though not all with equal distinctness. Where they are not consciously distinguishable, yet their unconsciously aroused traces influence the conscious content. The motor images are always of demonstrably greatest importance, in motor speech for determining it, in sensory for understanding it. Auditory, visual and motor types of individuals do not exist in the sense that one or the other element is present exclusively in any of their verbal images; they consist only in the prominence of one element over the others, all of which must be present.

E. B. Delabarre.

Brown University.

Ueber das Gedächtniss für Sinneswahrnehmungen. W. von Tschisch. Dritter Internat. Congress f. Psychologie. (Munich, J. F. Lehmann, 1897.) Pp. 95–109.

This paper, read at the Psychological Congress, is a report of several investigations upon memory carried on at the Dorpat Labora-

tory. The questions taken up were the space sense (Raumsinn), position sense (Ortssinn), active and passive muscle sense, active movement, sight, sound intensity and tones. These were studied by different investigators, under the direction of Professor von Tschisch, who took personal charge of the one on sound intensity.

The method pursued, with two exceptions, was that of Right and Wrong Cases. For the sense of space, distances on the skin were used as stimuli; for the muscle sense, weights; for sight, light impressions of different intensities; for sound intensity, sounds of the same pitch but different intensities; and for tones, sounds of the same intensity and different pitches. The stimuli were given in pairs; the subject was required to designate the louder, higher, brighter, etc., as the case might be. The proportion of right answers to wrong was noted, judgment of equality being ruled out.

Having determined a pair of stimuli which could be distinguished correctly by the subject in about 70 cases out of 100 when given in close succession, this pair was adopted for subsequent tests, in which an interval of time was made to elapse between the two stimulations. The interval, at first short, was increased by empirical steps (which varied in the different investigations) until some marked diminution of the percentage of right answers, usually below 50%, was obtained. When necessary to prevent the memory from lingering over from one trial to the next, different pairs of stimuli were used alternatively in the same series. The interval of time at which a marked falling off in the percentage of right answers occurred was taken as measure of the strength of memory in each case.

In two investigations in which the method of Mean Errors was used, the same general procedure was employed; the interval of time at which a marked increase in value of the mean error occurred was made the measure of the strength of memory.

The space sense was investigated by means of a pair of compasses, the place chosen being the right forearm. 70 mm. was taken as normal distance, but tests were made for greater and lesser distances as well. For the sense of position, the left forearm was chosen; a point was touched by the experimenter with a pencil, and the subject endeavored to touch the same spot with another pencil held in his right hand; a screen prevented the arm from being seen by the subject, who was allowed to 'feel around' for the spot after touching. In this investigation the method of Mean Errors was used. The muscle sense was investigated by means of copper cylinders, of uniform size and varying weight. For the passive muscle sense these were laid upon

the palm of the subject's hand for the space of three seconds and then removed; for the active muscle sense they were placed by the investigator between the subject's thumb and fore-finger, and raised by him. Active movement was investigated by requiring the subject to move his hand slowly to the right or left. A thin stick attached to the hand, with a pointer extending upward to a scale, served to mark the angle of flexion; in the first of each pair of experiments the length of movement was regulated by a knob which the hand encountered; this obstruction was then removed, and the subject required to repeat the movement. The method of Mean Errors was employed here also. The investigations in sight made use of the shadow cast by a steel bar upon a white surface, with varying distances of the light-source. For sound intensity, steel balls were dropped from varying heights on a wood surface. The last study was one on tone differences; five tuning-forks were employed, which differed by four vibrations, from 436 up. For musical subjects differences of four vibrations were used; for unmusical subjects differences of eight vibrations were chosen, as this was nearer their perception threshold.

Comparing the results of the several investigations, Professor von Tschisch finds the memory for space to be weakest; it falls off in exactness the soonest. Memory for position and the muscle sense is somewhat better developed. Memory for active movement and the higher senses is decidedly stronger. The memory for sight and sound, and in the latter sense for intensity and quality, appears to be about the same; in these three the percentage of right answers falls from 70 to about 50 in 15 minutes, for normal individuals.

H. C. WARREN.

PRINCETON.

Experimentelle Studien über Associationen. I. Theil. Die Associationen im normalen Zustande. GUSTAV ASCHAFFENBURG. Leipzig, Engelmann. 1895. Pp. 95.

Dr. Aschaffenburg's aim in his experimental study of association is the establishment of a method of diagnosis in cases of nervous disease. The published part of his work deals with normal associations as basis for the later study of neurasthenic cases. The methods employed are the simple ones of older experimental tests. A word is pronounced to the subject who responds by writing down or by pronouncing the first suggested word; or (in one form of the experiment) by writing down, as quickly as possible, the first hundred words occurring to him. There are 4,400 single cases of association, with

17 subjects, in series usually of 100, but occasionally of 200 or of 50 words. In 2,300 of these single cases the associated word is pronounced, and the time intervening between beginning of stimulus and beginning of reaction is measured by the use of Cattell's lip-key, connected with a Hipp chronoscope. Verbal associates only are studied, without reference to the accompanying images, usually visual, whether these amplified the suggested words or differ from them.

Dr. Aschaffenburg's theory and terminology are frankly Herbartian; his classification is first the ordinary distinction of what Wundt calls External and Internal Association, and then a more detailed division, mainly on the basis of that of Kräpelin. The problem of the experiments is the assignment of every associated word to its proper class, and the author emphasizes, with praiseworthy iteration, the absolute need in work of this sort of the coöperation of the subject, who alone can decide whether a given association is, for example, one of 'subordination' or of 'predicative relation.' The results of this detailed classification are, however, very meagre and justify no definite conclusions, as Dr. Aschaffenburg, who is most modest in his claims, very freely acknowledges. This failure supports the writer of this review in the conviction that the results of such minute classification of material so elusive, are always incommensurate with the doubts and difficulties of the undertaking. On the other hand, the experiments clearly justify the ordinary observation that associations of the concrete, matter-of-fact variety predominate strongly over the more focalized and abstract, for an excess of external over internal associations is noticed with every subject and in every form of experiment. A slight decrease in time also distinguishes these external associations.

The most significant outcome of the experiments is perhaps their demonstration that the absolute reaction-time of a subject affords no important psychic test, since the reaction-times of different normal individuals vary in so marked a degree (p. 67). While, for instance, the average association-time of eight subjects varies between 1,180σ and 1,426σ, it falls in the case of one subject to 927σ, and rises with another to 2,151σ, though the conditions are the usual ones, and though there is no marked individual eccentricity to explain the divergence. Dr. Aschaffenburg properly insists, therefore, that length or brevity of reaction-time cannot be supposed to distinguish the diseased from the sound subject or the abnormal from the normal state. Conditions of fatigue or of emotional disturbance do, however, affect the associations by occasioning interruptions in the continued series, and by augmenting the tendency to associate words through their sound—

alliterative or rhyming words—and to respond by mere disconnected repetitions of former words. From the consideration of these characteristics of association Dr. Aschaffenburg will proceed to his study of association in abnormal subjects. Incidentally, the monograph treats other topics: community of associations, accepted as an evidence of the lack of originality (p. 92); the difficulty of assigning with certainty the period of life to which associated images date back (p. 87); and finally, the grammatical type of associations (p. 82). The associated words, as Kräpelin had indicated, are chiefly nouns, but three of the thirteen subjects respond with a large number of verbs, while another often associates adjectives. The author is unable, however, to trace these types, which correspond with those suggested by Münsterberg (Beiträge, IV.), to any characteristics of individual thought.

MARY WHITON CALKINS.

WELLESLEY COLLEGE.

Ueber die Wahrnehmung von Druckänderungen bei verschiedenen Geschwindigkeiten. GEORGE MALCOLM STRATTON. Philosophische Studien, Band XII., Heft 4.

Dr. Stratton's experiments seem to have been carefully made, and are discussed with considerable acumen. Were it not for what is, perhaps, a fundamental error in the interpretation of results, the work would be far above the average thesis in psychology.

The first of the three sets of experiments made by the writer was on the accuracy of momentary pressure changes. The method used was that of minimal changes. The stimulus was applied by a system of levers to the little finger. The results of 560 determinations show clearly that a change in the pressure could be perceived before the direction of the change, and that the perception of an increase of pressure is more accurate than that of a decrease. Weber's law was found to hold for 75–200 g.

In discussing these results Dr. Stratton concludes that the process involved is similar to that of absolute threshold perception rather than that of difference discrimination. The greater accuracy of perception for increase of pressure is ascribed to the intensifying effect of attention, the writer rejecting the fatigue hypothesis of Hall and Motora, who had observed the same phenomenon.

In interpreting his experiments Dr. Stratton assumes that the stimulus is pure pressure, and goes so far as to regret that movement cannot be entirely excluded. Does he suppose that a sensation of

pressure is possible without movement? The fact is, as can be shown by the simplest experiments, that constant pressure, if below the pain threshold, soon ceases to effect consciousness. Pressure without movement cannot, therefore, be considered a stimulus at all, or classed together with other sensory stimuli. Gustatory and olfactory stimuli, it is true, seem to lose their effect gradually, but this phenomenon is easily explained by fatigue. Fatigue cannot, however, be caused by a pressure so small that we can hardly perceive it. Dr. Stratton's assumption is all the more remarkable in view of his recognition of the resemblance of the process to that of threshold perception. He himself observed that the change in pressure seemed to him but a slight touch of the stimulated surface.

In another series of experiments the writer investigated the effect of variations in the rate of change on the accuracy of perception. The rate of change of the stimulus was regulated by the application of Archimedes' law of fluid pressure, as in the other experiments. The method of minimal changes was used. The least perceptible increase was found to be inversely related to the rate of change, a result exactly the opposite of that obtained by Hall and Motora. But instead of rejecting contemptuously the work of the previous investigators, as many experimental psychologists would have done, Dr. Stratton repeated the experiments of Hall and Motora and got the same results. The discrepancy between the results of the two sets of experiments must, therefore, be due to the methods employed. The increase of the threshold from decrease of the rate of change is explained by the effect of attention and the conditions of the observer's reaction. In support of this view the writer offers considerable evidence. The opposing results he explains on the supposition that differences are more easily perceived, the closer the objects to be compared. How this applies to continuous changes is not clear. Had the writer not started with what is, I believe, a radical misconception of pressure phenomena, he would not have rejected so hastily the physiological interpretation.

HAROLD GRIFFING.

NEW YORK.

Mesure de la Clarté de quelques Representations Sensorielles. M. FOUCAULT. Revue Phil., Dec., 1896.

The problem set out for investigation is the determination of sensible discrimination for weight or pressure. The method employed is that of right and wrong cases. Small boxes of uniform size were used, in which the different weights were placed. The experiments

were tried on the back of the hand, always in the same place during same series. The subject was not permitted to see the weights, the results were withheld from him, and the order in which the different weights were tried was made irregular. By clearness of a representation is meant the degree of certainty with which it can be distinguished from any other representation. Expressed quantitatively; if v represent the right cases, and d the wrong cases, the clearness will be expressed by the formula $\frac{v-d}{v+d}$. The clearness of the representation is not the same as its intensity. It varies with the fatigue, degree of attention, etc., and not always in the same direction as the intensity. The functional relation between clearness and intensity has not been determined experimentally, but it may be said that, in general, the greatest clearness is obtained from *mean* excitation. M. Foucault's problem then is to establish a 'clearness-scale,' proceeding from the point where the two weights are not accurately discriminated. The utility of such a series of experiments is: (1) the determination of the variability of sensible discrimination. (2) A clearness scale will afford a means of comparison between senses qualitatively different. (3) "The determination of the mean clearness for a certain kind of perception, will furnish also a measure of sensibility for that stimulus." (4) Such a study furnishes an analysis in quantitative terms of different mental functions. Here we determine the function directly by the relative ease with which the mind discriminates. (5) Lastly, we shall determine the type of the subject. In this series of experiments M. Foucault makes out two distinct types of imagination.

The results are as follows: I. *Determination of mean clearness.* There were eight subjects. Four series of experiments were taken (540 ex. in each series, 240 on each subject). The weights varied from 18 to 20 gr. and were tried in two directions. 1. (The clearness varies for different subjects.) 2. This difference (1 to 4 gr.) is in all the cases less than one-third of the original excitation, which Fechner thought the threshold of difference for pressure. The proportion of right cases, and consequently the clearness, increases according as the excitation increases from 1 to 4. According to Fechner's theory the difference should be imperceptible. This confirms the results of Jastrow and Pierce. 3. The results present a number of negative cases, but even for the smallest difference of excitation the excess of right cases is appreciable so that the negative instances may be considered the exceptions, due to individual differences of the subjects, etc.

II. *Variation of clearness due to relative size of stimulus dif-*

ference. There are two cases: 1, where the difference increases; 2, where the difference decreases. In both cases "the mean clearness is greater according as the relative difference is greater."

III. *Determination of sensibility for pressure differences.* This is on the assumption that the relative clearness determined by the method of right and wrong cases will be a measure of discriminative sensibility as found ordinarily by the gradation methods. The results are obtained here by combining those of the previous experiments. It is a question whether they are exact enough to be of any great value. The sensibility of the two hands was not found to be appreciably different. Whence the conclusion is drawn that pressure sensibility does not depend at all upon the fineness of muscular development.

IV. *Conclusion relative to the imagination.* The subjects divide themselves into two classes: 1. Those who detect a decrease of stimulus most readily; 2. Those who detect an increase best. Throughout the experiments, in no case was the first stimulus replaced, so that always the judgment was between the second weight and the memory-image of the first. The sensibility is found to vary in either direction. It is not found to be greatest for the augmenting series, as Jastrow insists. (*Am. Jour. of Psy.*, Vol. I., 271 ff.) To explain this difference of type, M. Foucault proposes the following hypothesis: In the change from the perceived image to the memory-image, the latter will be sometimes greater, sometimes smaller than the original image. This change is due to the imagination. There are then two types of imagination, one tending to enlarge memory images, the other diminishing them. The coefficient of this variation is determined as follows: If *Ca* represent the mean clearness for the increasing series and *Cd* for the decreasing series, $\frac{1}{2}(Ca + Cd)$ will be the mean clearness of perception. In the case where *Ca* is greater than *Cd*, on the hypothesis the *Cd* clearness is less because the comparison image is diminished. So the imagination coefficient will have a negative sign and will equal $Ca - \frac{1}{2}(Ca + Cd)$ or $\frac{1}{2}(Ca - Cd)$. The reverse case will be where the *Cd* clearness is greater than the *Ca* clearness and the coefficient will equal $\frac{1}{2}(Cd - Ca)$. No direct relation is discoverable between the mean clearness, or sensibility, and the coefficient of imagination-variability, so that as one (mean clearness) increases the other tends to disappear. Conditions of fatigue, distraction, practice, etc., which affect sensibility, do not influence appreciably the imagination coefficient. This was tested carefully for practice.

This positive and negative character of imagination enables us to explain the negative cases which have been mentioned. The person who has a tendency to an enlarged memory-image will perceive with most clearness differences which are in the negative direction, and will have, consequently, a tendency to more false cases in the augmenting series, where the differences will be in favor of the *first* impression. And conversely, where there is a tendency to diminish the image, the greater error will be in connection with the diminishing series, where the difference will be in favor of the *second* impression.

J. M. TROUT.

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LOGICAL.

Manual of Logic. J. WELTON. 2d Edition. London, University Correspondence College Press. New York, Hinds and Noble. 2 vols. Pp. 411, 292.

This is a second edition of this work, revised and largely recast. Much of the historical matter of the first edition has been omitted. The author has endeavored also in this edition to give greater prominence to the distinctions of thought which underlie the distinctions of language with which the traditional logic is concerned. The two volumes cover the ground of deductive and inductive reasoning in a very exhaustive manner, dealing with the important questions of the theory of logic as a careful exposition of the technical rules and praxis. This treatise should prove a valuable help to teachers of Logic. It is adapted for such a purpose rather than for use as a text-book. The author's very thorough elaboration of his subject has lead him, at times, to rather a diffuse treatment, notably the chapters on Opposition, on Eduction, on Figure and Mood, and, in general, the more technical parts of formal Logic.

Ueber die sogenannte Quantität des Urtheils. OTTO SICKENBERGER. München, 1895.

This is a Doctor's dissertation presented to the University of Munich. It is, in the main, an historical survey of the logical doctrine of the quantity of the judgment. The author traces the discussion from Aristotle to the present, with special mention and criticism of the positions of the modern logicians, especially Lotze, Brentano, Sigwart, Wundt and Erdmann. He concludes with a short chapter

outlining his own position, being the constructive portion of his work, which is evidently secondary to that of historical exposition and criti cism. He divides judgments primarily into universal and individual, with an intermediate class, which, as Lotze, he designates by *this* A etc., to distinguish from all A's and some A's.

In this intermediate class he finds an illustration of a principle which he regards as fundamental to the true conception of judgment, namely, that the distinction between universal and individual lies in thought and not in the objects themselves. For the judgment of the form this A etc., may be regarded as universal or individual, according as the point of view is shifted from the individual to the universal aspect. Moreover, some objects essentially different may, in thought, be the same if regarded exclusively from the standpoint of their identity, disregarding completely their differences; so, also, one and the same object may be represented in thought as two instead of one, as it is regarded, first in its individual, then in its universal character and relations. There is, however, an impression left upon the reader that there is a sundering of concept from reality. Differences in thought, owing to a shifting of the subjective point of view may, nevertheless, be referred to the reality whence they emanate. The abstract must have some basis of reference which rests upon the real and concrete. The hypothetical universal may swing clear of reality in a sense; that is, that its expressed condition may never be realized. Yet the ground of the hypothetical relation thus expressed in thought must have a reference to reality; otherwise it may be only an imaginative connection, which, even if the conditions were to be realized, the alleged result would not follow.

Dr. Sickenberger's analysis of the various judgment forms is clear and exhaustive. On the whole, the chief value of the dissertation lies in its historical contributions.

JOHN GRIER HIBBEN.

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Outlines of Logic and Metaphysics. By JOHANN EDUARD ERDMANN. Translated from the Fourth (revised) Edition, with Prefatory Essay, by B. C. BURT. New York, The Macmillan Co., 1896. Pp. xviii + 253.

In view of its well-known relation to the Hegelian school, of its first publication fifty-two years ago, and of the immediate interests peculiar to readers of THE PSYCHOLOGICAL REVIEW, it is probably unnecessary to enter here upon a detailed discussion of the contents of

this work. Other considerations, however, present themselves. In face of them it is not easy so to review the book as to do justice to the solid work expended upon it by the translator. For, on the one hand, Dr. Burt's translation is careful and accurate, though far too literal to admit of freedom from stiffness, so indispensable in an introductory manual. Further, his prefatory essay is a competent piece of writing, displaying commendable intimacy with the alterations upon the Hegelian logic made and proposed by disciples of the first generation, like K. L. Michelet, J. E. Erdmann, Rosenkranz and C. H. Weisse. On the other hand, it may very well be questioned whether the book was the right one to translate. Written within a decade after Hegel's death, it bears abundant traces of the atmosphere from which it sprang. And, in these circumstances, it is a very open question whether the student's purpose would not be better served by study of the smaller logic of Hegel in Professor Wallace's translation, especially as it is presented with so much valuable apparatus in the way of elucidatory comment. Yet again, modern logic has not been stationary, and a satisfactory 'Introductory Text-Book' ought to bear a date later than 1864, that of the edition from which the present version is made. On the whole, then, it must be said that between the classical work of Hegel himself and Dr. Bosanquet's recent *Essentials of Logic*, no place remains for this translation. Moreover, its terminology is as harsh as Hegel's own, and is not lit up by those illustrative flashes which the reader expects from the author of the *Phänomenologie*. Of course, criticism of this kind raises the whole question of translating. And it must be said that sufficient discrimination is not always shown. The translator needs common sense for his selection as well as knowledge for his rendering. Dr. Burt has the latter in plenty; one cannot think that he has in this case weighed the circumstances which will certainly militate against his work as a students' manual. It ought to be added that the 'get up' of the book is little creditable to the publishers; and in this matter Messrs. Sonnenschein, of London, and not the Macmillan Company, are to blame.

R. M. Wenley.

University of Michigan.

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NOTES.

THE American Psychological Association has joined the Naturalists and Affiliated Societies in accepting the invitation of Cornell University to hold the next annual meeting in Ithaca.

PROFESSOR J. G. HIBBEN has been made full Professor of Logic on the Stuart foundation, and Mr. J. F. Crawford has been appointed Demonstrator in Experimental Psychology, both in Princeton University.

THE *Année psychologique* may hereafter be had from Mr. G. Stechert, the New York bookseller (9 E. 16th St., N. Y.), who will act as the American agent.

HERR CARL WINTER will issue, from the Universitätsbuchhandlung in Heidelberg, a jubilee edition of the *Geschichte der neuern Philosophie* of Kuno Fischer, who celebrated his 50th anniversity in March, 1897. The nine volumes will appear in 40 monthly parts at 3 M.

WE are glad to announce that Professor Hugo Münsterberg, who is one of the coöperating editors of the REVIEW, is to resume the duties of his position at Harvard University in the autumn.

THE members of the American Psychological Association are reminded of the summer meetings of the American Association for the Advancement of Science in Detroit, Mich., beginning August 9th, and of the British Association for the Advancement of Science in Toronto, Canada, beginning August 17th. Provision is made by both Associations for papers in experimental psychology, under Section H (anthropology) of the American Association and under Section I (physiology) of the British Association. By action of the authorities of both associations the members of the American Psychological Association are cordially invited to attend the meetings and become members. All members wishing to join the American Association are requested to notify Professor J. McK. Cattell, Columbia University, New York City, who will furnish information regarding that Association.

All members intending to present papers at the meeting of the British Association are requested to send abstracts of their papers before May 15th to Dr. A. Kirschmann, University of Toronto, Toronto, Canada, Secretary of Section I, from whom full information regarding the meeting can be obtained.

We record with regret the death, on April 11th, of Professor Edward D. Cope, professor in the University of Pennsylvania, editor of the *American Naturalist*, President of the American Association for the Advancement of Science, and eminent for contributions to paleontology, zoölogy and a wide range of natural science. Professor Cope was greatly interested in psychology, and, as is well known, made much use of psychological factors in his contributions to the theory of evolution.

Professor J. Mark Baldwin has been awarded the gold medal offered by the Royal Academy of Arts and Sciences of Denmark for the best work on a general question in Social Ethics. There were nine memoirs in the competition written in four languages. Professor Baldwin's work was entitled 'The Person Public and Private' and is in part the volume of 'Social and Ethical Interpretations of the Principles of Mental Development' which has been announced for early publication by The Macmillan Co.

Dr. W. B. Pillsbury has been promoted to an instructorship in experimental psychology in Cornell University.

The interest of the Frohschammer fund of the University of Munich, amounting to $400, is offered for an essay on 'A Psychological Analysis of the Facts of Volition,' which must be presented before October 1st, 1899.

Charles Scribner's Sons have in press a work on *Philosophy of Knowledge*, by Professor G. T. Ladd, Yale University. The subjects treated may be seen from the titles of the chapters, which are as follows: I., The Problem; II., History of Opinion; III., History of Opinion (continued); IV., The Psychological View; V., Thinking and Knowing; VI., Knowledge as Feeling and Will; VII., Knowledge of Things and Knowledge of Self; VIII., Degree, Limits and Kinds of Knowledge; IX., Identity and Difference; X., Sufficient Reason; XI., Experience and the Transcendent; XII., The 'Implicates' of Knowledge; XIII., Scepticism, Agnosticism and Criticism; XIV., Alleged 'Antinomies;' XV., Truth and Error; XVI., Ethical and Æsthetical 'Momenta;' XVII., The Teleology of Knowledge; XVIII., Knowledge and Reality; XIX., Idealism and Realism; XX., Dualism and Monism; XXI., Knowledge and the Absolute.

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Further particulars will be found in the Announcement of the Division of Philosophy, Psychology and Education, which may be obtained from the Secretary of the University.

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